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LECTURES
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LECTURE XXXVIII.

A SKETCH OF THE NERVOUS SYSTEM—THE ORIGINS AND FUNCTIONS OF THE NERVES OF PECULIAR SENSATION, THE OLFACTORY, OPTIC, AND AUDITORY; AND ALSO OF THE NERVES OF PURE MOTION, THE THIRD, FOURTH, SIXTH, AND TWELFTH PAIRS.

I HAVE observed in a former lecture, that the cerebral and the spinal nerves present themselves symmetrically, that they are arranged in pairs, one on each side of the mesian line, that runs from the anterior portion of the brain to the cauda equina. I will first enumerate them in the order in which they present themselves at the base of the brain; rapidly describing their origin, destination, and function. We shall then, perhaps, be able to class them according to their respective functions, and thus, perhaps, get rid of a little of the mystery which has been supposed to encircle the nervous system. Having done this, I shall enter at some length into the consideration of the office of each, and that will be the time for me to endeavour to justify my departure from the nomenclature and physiological exposition hitherto adopted by veterinary writers.

Description of the Cerebral Nerves according to their Situation at the Base of the Brain.—Commencing anteriorly, we find,

1. *The Olfactory* nerve arising from the corpora striata and callosum; passing through the cribriform plate of the ethmoid bone, and distributed on the membrane of the nose—*a nerve of peculiar sensation*, and its minute ramifications, affected only by the odoriferous particles of bodies.

2. *The Optic* nerve arising from the thalamus nervi optici, and connected by medullary striæ with the corpora quadrigemina,—pursuing its course to the back of the eye, piercing the sclerotic coat, expanding over the choroid coat, and forming the retina; *a nerve of peculiar sensation*, and affected only by the particles of light.

3. *The Motor Oculi*, from the middle of the crus cerebri to all the muscles of the eye, except the superior oblique and the abductor—*a nerve of voluntary motion*.

4. *The Patheticus*, from a little sulcus on the crura cerebri at the base of the testes to the superior oblique muscle,—*a nerve of voluntary motion*.

5. *The Trigeminus*, evidently proceeding from two roots: the inferior one from the crus cerebri—the superior one from the crus cerebelli. The superior one forming or passing through a ganglion—the inferior one unconnected with the ganglion, but uniting with the other branch immediately beyond the ganglion, and both contributing to form one compound nerve distributed over the face,—*a nerve of common sensation and of voluntary motion*.

6. *The Abducens*, from a transverse sulcus between the pons varolii and motor root of the eighth pair (the portio dura of the seventh), and going partly to the retractor, but principally to the abductor muscle of the eye—*a nerve of voluntary motion*.

7. *The Voluntary and Organic Motor Nerve of the Face.*—The portio dura, springing evidently from two roots; the first to be traced from the central column of the inferior surface of the medulla oblongata, and the other from the head of the lateral column, or corpus olivare, if it might be so termed in the quadruped;—united together as they escape from the brain, and forming a compound nerve, *the voluntary motor nerve of the face, and directing also those motions, partly voluntary and partly involuntary, which are connected with the respiratory system of organic life.*

8. The *Auditory* (the portio mollis of the seventh), traced from the floor of the fourth ventricle; passing down the side of the medulla oblongata; entering into the same sheath with the eighth (the portio dura);

escaping through the foramen auditorium internum, and spreading its pulpy ramifications over the internal chambers of the ear, — *a nerve of peculiar sensation*, and affected only by the vibrations of the air.

9. The *Glosso-Pharyngeus*, arising from the lateral column of the medulla oblongata by several little filaments, partially ramifying over the constrictor muscles of the pharynx, but chiefly expended in the base and substance of the tongue. *An organic motor nerve* associating the action of the muscles of the pharynx and tongue, with the discharge of the function of respiration and deglutition.

10. The *Par Vagum* of our anatomists— the *pneumo-gastric* nerve of the French, but, more properly, the *cerebro-visceral organic motor nerve*. It arises from the lateral column of the medulla oblongata, by numerous filaments, a little posteriorly to the glosso-pharyngeus, and escapes from the cranium in the same sheath with it. After distributing several branches to the pharynx, the larynx, and the œsophagus, it gives large and singularly involved plexuses to the heart and the lungs; and having entered the abdomen, its ramifications on the stomach are numerous and complicated. Some plexuses go to the liver, and an important branch, uniting with the grand organic nerve of the abdomen, helps to form the similunar ganglion,—*an organic motor nerve*.

11. Included in the same sheath with the glosso-pharyngeal and the cerebro-visceral, is a singular nerve, which anatomists have agreed to call the *spinal accessory nerve*. It is formed by the union of several minute filaments derived from the lateral column of the spinal chord, some of which may be traced as low as the fifth cervical nerve. Having, in the manner I shall hereafter describe, reached the cerebro-visceral and glosso-pharyngeal, it neither identifies itself with them, nor communicates with the substance of the brain, but turns round and escapes in the same sheath with those nerves, and, soon separating from them, is distributed over some of the muscles of the neck and shoulder, associating with them in the involuntary discharge of the function of respiration, and therefore being *an organic motor nerve*.

12. Still posteriorly, on the medulla oblongata, but rising from the central column of its inferior surface, we find the *linguales* escaping through the anterior condyloid foramina, running between the pterigoideus, and the larynx, penetrating into the substance of the tongue, and giving motion to that organ. *It is a voluntary motor nerve.*

Here ends our brief catalogue of the cerebral nerves; but our subject would not be complete if I did not glance at the origins of the nerves along the course of the spinal chord. At the very point where the medulla oblongata terminates in the spinal chord, we find the first pair, *the sub-occipital*, and we may take it as a specimen of all the rest. Numerous little filaments arise, from the longitudinal sulcus between the central and lateral columns on the inferior surface of the chord, but which are derived from, or may be traced to, the central column; they approximate and unite, and form a nervous chord. Other filaments spring from the corresponding sulcus on the superior surface, but they are not so numerous: and they are larger and bolder in their origin. They also approximate and unite, and form a nervous chord; but at the point of union, and still within the dura mater, these latter form a ganglion,—a little swelling or knot, — immediately beyond which, the chord shrinks again to nearly its former bulk; and then, and still within the dura mater, or at the moment of escaping from it, the two chords unite and form a perfect but compound nerve, of which I shall say no more at present, than that it combines the properties it derived from these central columns on the inferior and superior surfaces of the spinal chord, and is *a nerve of sensation and voluntary motion*. It is distributed to the neighbouring parts, and it anastomoses with those above and below, for important purposes, which will be the subject of future consideration. Given out at each articulation of the cervical bones, as we proceed down the neck we find one of these compound nerves; and towards the lower part of the neck, we observe filaments from the fourth, fifth, and sixth cervical nerves, uniting to form a singular nerve, which takes its course down the neck, without anastomosis, and traverses the chest, and ramifies upon the diaphragm. This *phrenic nerve* is the motor nerve of the diaphragm,—the grand agent in respiration, and, therefore, we have no difficulty in denominating it *an organic motor nerve*. I shall have to show, that it is not improbable, that although the filaments which compose it are derived from the cervical nerves, yet they may actually have their origin from that portion of the spinal chord which seems to be devoted to the purposes of organic life.

The dorsal, lumbar, and sacral nerves, have the same structure, and discharge the same function. They are all nerves of sensation and voluntary motion; and those of the extremities, derived from plexuses of the cervical and dorsal, and the lumbar and the sacral, possess the same character.

The Ganglial Nerve.—There is one nerve wanting to complete our list. It belongs neither to the cerebral nor the spinal system, and seems in its function to be independent of both. At the base of the cra-

nium, and in front of the atlas, I find a pyriform reddish body, which gradually contracts and terminates in a nerve. I inquire not now into the origin of this ganglion, but a superficial glance at the nerve tells me that it is performing some important office. It is connecting itself with the cerebro-spinal, and with every cervical nerve, but more particularly it is forming complicated plexuses on every neighbouring blood-vessel. I trace it particularly on both the external and the internal carotid, and I follow it in the subdivisions of these vessels, until, from the minuteness of the vessel and the pulpiness of the nerve, it eludes my sight. Hereafter I shall have to trace the course of the nerve in the thorax, forming, with the cerebro-visceral, a plexus, or rather an investing membrane around every vessel of the heart and lungs; and then having reached the abdomen, and, in the semilunar ganglion combined its own influence with that of the cerebro-visceral and the phrenic, it becomes the sun or centre of organic nervous power, diffusing its radiations over every artery and absorbent, and gland and ganglion—everything connected with secretion, nutrition, and life—itself the very principle of life and action—the soul of the organic system. It was termed, before its character and power were suspected, the *sympathetic nerve*, because it seemed to connect the whole system together; it is denominated by others the *ganglial nerve*, from its supposed origin either in the superior cervical, or the semilunar ganglion; but it would more properly be designated *the great organic nerve*—the *secretory, nutritive, chemical*, while the cerebro-visceral is the *motor* organic nerve—the power which presides over, and effects the changes in, that fluid which the motor nerve keeps in circulation. But we are not yet quite prepared for this.

My object was to give you a kind of bird's-eye view of the nervous system, that we might perhaps be enabled so to class its different portions, and trace its connexions, as to remove much of the mystery that has erroneously been supposed to hang over the subject.

Divisions of the Nervous System.—One division is plain and palpable—*the animal* and *the organic*—that which is concerned in the conveyance of impressions to the brain, and volitions to the muscles, and that which is connected with respiration, circulation, digestion, and life itself. The six first of the cerebral, the eighth (portio mollis), and the twelfth (the linguales), and all the spinal nerves are those of *animal* life. All the others except the seventh (portio dura) are nerves of *organic* life. Both the animal and the organic nerves admit of a simple subdivision into those of *pure sensation*, concerned alone in transmitting impressions to the brain as the first, second, and seventh; or *pure motion* conveying the volitions of the mind; as the third, fourth, sixth, and twelfth (linguales)—or, in accomplishing both purposes, as the fifth pair within the cranium and all the spinal nerves. The division of the organic nerves is not so evident; we have, however, plainly enough the simple motor-organic nerve in the ninth (the glosso-pharyngeus), the eleventh (the spinal accessory), and the phrenic—and we shall cautiously inquire whether we do not find it also in the tenth (the par vagum), and the chemical and vital one in the ganglial nerve; while, plainly enough, the seventh pair occupies a neutral ground, or belongs both to animal and organic life—it is both the *voluntary and the organic motor nerve of the face*. When we have made ourselves masters of this theory of the origins and functions of the nerves, we shall not have much difficulty in unravelling many of their anastomoses, and comprehending the wise and benevolent purposes that are accomplished by them.

We will commence with the animal nerves, and, first of all, with those of pure or peculiar sensation.

The Olfactory, or First Pair of Nerves.—The nerve which first presents itself at the base of the brain, if we commence anteriorly, is *the olfactory nerve*. It arises partly from the *corpus striatum*; but a medullary tractus, terminating in it, and evidently giving one origin to it, may be traced to the base of the *corpus callosum*.

Its Character as a Nerve of peculiar sensation.—Whether we trace its origin to the corpus striatum or the corpus callosum, it is derived from the *superior portion* of the medullary matter at the base of the brain—that which would be a continuation of the superior surface of the spinal chord if so far prolonged. If we follow either root to the corpus callosum, the olfactory nerve is evidently referable to the *centre* of that superior surface, and there is nothing inconsistent with the same derivation, when we attribute it to the corpus striatum. It proceeds in a manner *bodily* from the substance of the brain—it is an apparent *prolongation of the medullary matter of the brain*.

Comparative anatomists should not describe it as a mere mamillary process—"the ashen-coloured termination of the brain abutting upon the ethmoid bone." Although the nervous chord is not separated from the brain to so great an extent as that of the optic, or even the auditory nerve, and not nearly so much as we find it in the human subject, yet it has decidedly left the brain before it reaches the cribriform plate. *Its course is short, and it has not*

a single anastomosis. I would beg you to pay attention to these peculiarities, these characteristics (as I shall, by-and-by, be justified in calling them) of a nerve of *peculiar sensation.* Its minute ramifications I have said are affected only by the odoriferous particles of bodies.

Size of the Nerves.—Observe the development of this nerve, bearing a beautiful proportion to the necessities of the animal. First, compare it with the same nerve in man—in him simply connected with pleasure—in the brute with life itself. In a brain not more than half the size of that of the human being, namely, that of the horse, the olfactory nerve is four times as large as in man. Compare its bulk in our different domesticated animals. In the horse it is large, for in a state of nature it affords his only warning against poisonous plants—but, almost all over the world, he has become our stabled servant, comparatively rarely sent to collect his own nutriment amidst the herbage of the field, and having the greater part of his food provided for him. The ox is oftener driven to shift for himself, or, if worked by day, he is usually turned out at night, and needs a somewhat acuter sense of smell. Observe, that although his brain is but little more than half the size of that of the horse, the olfactory nerve is nearly as large. In the sheep and swine it is comparatively as large as in the ox, or even more so; and in the dog you cannot fail of remarking its still greater comparative bulk.

Peculiarities about them.—The olfactory is, next to the fifth, the largest of the cerebral nerves, and it is the softest in its texture, nevertheless its fibrous structure has been demonstrated, and it is the excessive looseness and thinness of the neurilema that gave it its pulpy appearance. It has a singular cavity in it, the use of which I am unable to state. That cavity is a prolongation of the anterior cornu of the lateral ventricle; it terminates however in a blind pouch before the nerve reaches the ethmoid bone, and there is consequently no channel by which, as was formerly supposed, any fluid can be conveyed from the brain into the nose.

Termination.—In company with, and under the real mamillary process, it abuts upon the cribriform or thin perforated plate of the ethmoid bone, and sends its pulpy fibres through it, which are visibly spread over the upper part of the septum and superior turbinated bone, and which probably extend over the whole of the nasal cavity.

The Olfactory Nerves in Birds.—In our occasional patient, the feathered biped, the olfactory nerve comes nearer to a mamillary process, for it arises from the very point of the anterior termination of the brain, and seems to be a mere continuation of it. Having left the cranium it enters into a bony canal, and so passes through the orbit which interposes; and it is finally distributed, not on the ethmoid bone, for that is in a manner wanting in the bird, but on the pituitary membrane covering the superior *conchæ* of the nose.

The Optic, or Second Pair of Nerves.—On the crura cerebri, when they first appear at the base of the brain, there are seen two prominent medullary chords, winding their way over the crura in a direction inwardly and anteriorly. They first emerge from under the hemispheres, opposite to the corpus albicans, and they meet each other immediately anterior to the infundibulum and the pituitary gland.

Their Character as Nerves of a peculiar sensation.—The medullary chords of which I have just spoken—the *tractus optici*,—can be traced to the *thalami nervorum opticorum* in the centre of the brain; they are, in fact, a continuation of the thalami; or the thalami contract, and are prolonged into these medullary chords. I have also said that they are connected by medullary striæ with the corpora quadrigemina, and particularly with the nates. Now the thalami and the nates evidently occupy that portion of the base of the brain which would correspond with the *superior surface of the spinal chord*; and they occupy a *central* situation upon that surface. The optic nerves are also a palpable *prolongation of the medullary substance of the brain.* The course is *somewhat longer* than that of the olfactory nerve, but through the whole of that course, *they have not a single anastomosis.* They are also nerves of *peculiar sensation*, and their minute ramifications are affected only by the particles of light.

Peculiarities of the Optic Nerves,—Their Decussation.—I have shown you these medullary chords, winding their way over the crura, and meeting at the centre, and not only meeting, but apparently decussating—crossing. There is a certain identification of medullary matter of the two nerves, to secure the more perfect discharge of their function—to assist experience in correcting the errors which would arise from the somewhat dissimilar impressions made, or the pictures (if we so dare to call them) painted on the two retinæ, and to ensure perfect and distinct vision. The impression made by the odoriferous particles of bodies on the two nostrils, or the two olfactory nerves, are, in a state of health, precisely the same; the vibrations of the air, as they reach the tympanum of the ear on either side are the same; but from the construction of the organ of sight, the picture on each retina must differ, and that very materially, with regard to a near object. Look at any object first with one eye,

and then with the other, and you will understand what I mean. This partial mingling of medullary matter in the two nerves, was probably designed to prevent or to remedy, the confusion of vision which would otherwise result. We have, however, abundant proof that there is no actual decussation. This brain was taken from a dog that had been blind in the right eye for several years. Observe how the nerve on the right side is shrunk, and what a curious semi-transparent hue it has assumed. Our principal patient, the horse, is sadly exposed to inflammation of the eye, which too often destroys the sight: but there is this peculiarity about the disease, that when blindness is produced in one eye, the other is spared for a long time, and often for life. If we examine the brain of one of these horses, we uniformly find the nerve on the blind side shrunk; it obeys the law which governs every other part of the frame, and when it is no longer useful, it gradually withers away. It has been long settled, I believe, among human anatomists, that there is no decussation of the nerves. The examination of some Russian criminals determined the matter. Theft was in some part of that vast empire punished with the loss of an eye for the first offence, and death for the second. Some pirates who had suffered the first penalty, were afterwards convicted of a second offence and executed, and the nerve on the blind side was shrunk in every one of them. In birds, the optic nerves, which are exceedingly large, manifestly decussate; and in fish, and particularly in those with a bony skeleton, they cross each other without mingling at all.

Their Fibrous Structure.—The fibrous structure of the optic nerve is sufficiently evident in all our quadruped patients, and these fibres are so symmetrically arranged, as to leave a canal in the centre of the nerve, which had been observed by some old anatomists, but the function of which had been misunderstood. It contains *the central artery of the retina*, derived from the ophthalmic artery, which penetrate through the dura mater after the nerves leave the cranium, and enter with the optic nerve into the globe of the eye, for purposes which will be described when we treat of vision. This canal also contains the central vein of the retina.

The Reticulated Structure of the Optic Nerve.—When, however, I speak of symmetrical arrangement, do not misunderstand me; I do not mean to say, that the exact parallel direction of these fibres can always, or scarcely ever be traced. It would seem to be necessary in such a nerve as this, of peculiar sensation, that there should be an intercommunication of nervous influence and matter through its whole extent, otherwise accident or disease might, oftener than it does, cause strange confusion in the impression conveyed; therefore there is a continual exchange of minute branches in every direction going forward between the fibrils, in some nerves through their whole extent, in others more plainly to be traced in certain parts, and occasionally carried to such an extent, that the interior of the nerve presents only a complicated reticular appearance. It is so here; although we can trace the central canal plainly enough, the interchange of branches is so numerous, that the true parallel direction of the fibres cannot be distinguished.

Termination of the Optic Nerves.—Each nerve, after having separated from its fellow, pursues its course on its own side, escapes through the foramen opticum, and enters the cavity of the orbit; it there continues its path obliquely, penetrates between the muscles, and, particularly surrounded by the retractor muscle, reaches the inner and inferior and posterior part of the eye-ball, the coats of which it penetrates, and, entering the globe of the eye, it expands over the choroid coat, and forms the retina.

Sheathed in Dura Mater.—The dura mater quits the nerves as they emerge from the cranium, and they afterwards pursue their course surrounded and defended by their own neurilema. The optic nerve is, however, an exception to this rule, for the dura mater continues to encircle it until it reaches the outer or sclerotic coat of the eye. This, according to some, is for the purpose of forming the sclerotic coat, which certainly bears considerable resemblance to the dura mater; while the pia mater, according to the same anatomists, is expanded within the globe of the eye. More of this when we treat of the structure of the eye; at present I will only remark, that the difference in situation, and length of course, between this and the other two nerves of peculiar sensation, and the difference in function too, will sufficiently account for this prolongation of the dura mater. It is necessary that the impression or intelligence conveyed by a nerve of peculiar sensation, should be distinct and pure. The olfactory, the moment it quits the cranium, ramifies upon the membrane to which it is destined. We shall presently see, that the auditory nerve is defended by a bony canal until it arrives at the labyrinth of the ear. Nothing can cause confusion in the impressions which are conveyed along these nerves, but the optic nerve has to travel through the orbit before it reaches its point of destination, and is encircled by muscles of strong and rapid action; this dense covering, therefore, is necessary to protect it from that compression which might occasionally

interfere with the perfect discharge of its function.

The Optic Nerve in Birds.—The structure of this nerve is singular in birds. The fibrous conformation is evident; but these fibres are curiously collected into laminæ in birds of prey, and I think I have seen something like it in some of the wilder birds which we have partially domesticated, as the pheasant. The nerve cannot be dissected, as in the quadruped, into a collection of fibres, more or less numerous and complicated, but into a congeries of leaves; it seems like the folding of one continuous membrane, and the retina is the expanding of the plaits. We can connect this, as we shall have to do many other peculiarities in the eye of the bird, with the almost inconceivable acuteness of sight in birds of prey particularly, and of which natural history affords us a thousand proofs.

The Auditory Nerve.—This nerve is the eighth in order, reckoning anteriorly posteriorly. English anatomists have termed it the *portio mollis* of the seventh pair; I will venture, following the example of most continental anatomists, and for reasons that shall be stated hereafter, to call it simply the auditory or eighth pair of nerves.

Character as a Nerve of peculiar Sensation.—I find it on the floor of the fourth ventricle, which is not only a prolongation of, but may almost be said to be the *superior surface of*, the spinal chord: I trace it from the slit of the calamus scriptorius, and therefore from the *centre* of that surface. Its pulpiness and transparency render it now and then a little difficult to follow, and I mutilate or lose it in the dissection, but you here plainly perceive it passing down the side of the medulla oblongata, meeting the seventh nerve (the portio dura of the seventh), and then, in union with it, taking an outward direction, and entering the *foramen auditorium internum*. Its course there is short; it soon abuts upon a cribriform or perforated plate of bone closing the passage, through which it passes, and then dividing into various portions, spreads itself over the internal chambers of the ear, while there is not throughout the whole of its course *one anastomosis*. Its root cannot, like the first and second nerves, be traced to distant parts, for it at first appears on that track of medullary matter to which the others are ultimately referred. It does not seem so much a prolongation of a part of the brain, and yet it springs, as it were, *bodily*, and *at once*, from the floor of the ventricle. Human anatomists have spoken of fasciculi of fibres proceeding from above and below to form the root—something like strize I have seen; but the fibres, if they do exist, are indistinct.

There is a circumstance respecting one of the divisions of this nerve, that we have not observed in any portion of the first or second nerves, namely, that those fibres which go to the trumpet-like extremities of two of the semi-circular canals, pass through a little knot or ganglion. I must not at present draw any conclusion from this isolated fact, but I shall have hereafter to remind you of the ganglion on a part of the 8th nerve of peculiar sensation. The minute ramifications of this nerve are affected only by the vibrations of the air.

Comparative Bulk of the Auditory Nerve.—When I come to treat of hearing, I shall have to point out many peculiarities, accounting for the greater acuteness of this sense in the quadruped, than in the human being, arising from the external ear—the tympanum, the ossiculi, &c.; but at present I must only point out to you in each of the brains before us, the greater bulk of the auditory nerve compared with that in man; on the other hand, the adaptation of the human ear to the various modulations of tone, will be an interesting subject of inquiry.

Comparison of these Nerves of peculiar Sensation.—Now, then, Gentlemen, let us for one moment recapitulate. We have examined the three cerebral nerves of *peculiar sensation*,—those of smell, sight, and hearing. We have traced each of them to that portion of the medullary matter at the centre of the brain, which answers to, or may be considered as a prolongation of, the surface of the spinal chord;—we have further traced them to the centre of that surface;—we have observed, that they seem to be prolongations of the substance of the brain, or to spring bodily from that organ, and that through the whole of their course they have no anastomosis. To the two first of these peculiarities I shall hereafter recall your recollection, when I shall have to speak of the relative situation of nerves of certain functions—the prolongation of the substance of the brain—the long root that can be traced deep into the brain, seems natural enough, when we consider, that from these three nerves the animal derives almost all his knowledge of surrounding objects—all the materials for thought and improvement. It seems reasonable that these impressions should be conveyed securely and deeply, there to be preserved, arranged, and compared. They are as much identified with the mind and with animal life, as the well-protected organs of the thorax are with the inferior organic principle,—and the wisdom and benevolence of the arrangement by which these impressions of smell, and hearing, and sight, are preserved distinct and pure, and by which no anastomosis with other nerves is permitted, needs no illustration of mine. But

I proceed to other nerves, differently situated and of different function, viz. the nerves of pure motion.

The Motores Oculorum, or Third Pair of Nerves.—About the middle of the crura cerebri in the horse, (and to this animal, if an especial exception is not made, all these anatomical details refer,) and rather nearer to the corpus albicans than to the pons varolii, the third pair of nerves take their origin. They seem to rise superficially from the crura cerebri; they cannot be traced at all into the substance of the brain, except in a way that I shall presently notice, and are clearly referable to another surface of the spinal chord, or its supposed prolongation—the *inferior* one. Let me here remark, once for all, that from the difference of the attitude of the human being and the quadruped, the *anterior* surface of the spinal chord in man, is the *inferior* one in the brute; and the *posterior* one in the biped, is the superior one in the quadruped. These nerves spring from the *central* column of the inferior surface, that track which I have already traced under the pons varolii, and along the crura cerebri. These nerves, do not, however, rise *bodily* from the crura, but by *several minute fibriculi ranged in a line*, following the course of this medullary track. These fibriculi approximate to each other, and unite either before they penetrate the dura mater, or as they are leaving it, and form a nerve. This is very different from what we have observed in either of the nerves of peculiar sensation.

Course of the Nerve.—The motor oculi takes its course obliquely across the crus cerebri, and winds downwards, and enters the cavernous sinuses, and escapes through the foramen longum orbitale into the orbit; —there it divides, and the smaller branch goes to the levator oculi, while the larger subdivides, and goes to all the other muscles of the eye, except the superior oblique and the abductor, for the purpose of moving the eye in its socket. Then it is *a nerve of voluntary motion.*

Its Anastomosis.—As we follow its course we observe another circumstance in which it differs from either of the sensitive nerves, —it connects itself with other nerves. It communicates with minute branches of the ophthalmic division of the fifth pair, and more particularly unites with them that curious ganglion *the ophthalmic*, whence the iris derives its supply of nervous influence. I enter not into the cause of this yet. We shall know more about it by-and-by, for a thousand anastomoses will come before us, the great utility of which will be evident on the slightest inspection; but I content myself with pointing out to you how far we are departing from the character of a nerve of peculiar sensation.

The Pathetici, or Fourth Pair of Nerves.—I feel considerable difficulty in classing this nerve. It goes to the trochlear superior oblique muscle of the eye, and governs its motion, but whether it be a nerve of pure voluntary motion, or one associating the motion of the eye with some organic function, and probably that of respiration, I am not assured. It arises from a singular part of the brain,—from the crus cerebri at the base of the testes, and, consequently, from the superior surface of the prolongation of the spinal chord; it arises by filaments. I can plainly trace two, but it does not arise either from the centre, or the side, of the crus—more inclining, however, to the latter than to the former;—and when I make a longitudinal incision through the cerebellum, and lay open the whole of the fourth ventricle, I plainly see that it does spring from that portion of the crus which may be considered as a possible prolongation of the lateral column of the medulla oblongata. In this respect it differs from the voluntary motor nerve which I have just considered, and from every other voluntary motor nerve of the brain or the spinal chord. On the other hand, it curiously winds its way through the other nerves, and proceeds to this single muscle without an anastomosis; nor can I trace to the trachealis muscle any fibre from any voluntary motor nerve. This is very singular, for with regard to every other muscle (the visceral ones excepted), if there be communications from an organic nerve to associate the action of that muscle in the discharge of some vital function, there are also communications from some spinal cerebral nerve for the purpose of voluntary motion.

Difficulty about the Fourth Pair of Nerves.—Sir Charles Bell, to whom, more than to any other physiologist, we are indebted for our knowledge of the nervous system, considers this as a respiratory nerve, partly from its situation; and there I confess I am a good deal disposed to acquiesce with his opinion; but I do not understand the function which he assigns to it, of *relaxing*, not *contracting*, the trachealis muscle, and thus causing the eye to roll upwards, under the superior lid, in every forcible effort of respiration, as sneezing or coughing, and so tending to the security and preservation of the eye. I can form no clear conception of this *relaxation* of a muscle by the *direct* agency of the nerve that supplies it—nor can I see the great danger to which the eye is exposed in more than usually forcible respiratory efforts, nor a purpose sufficiently important to lead to the introduction of a separate nerve, and from so singular an origin. I must, therefore, at present class it with the nerves of voluntary motion. I

know it to be a voluntary motor nerve as it regards the usual action of the superior oblique muscle, and yet there is something about this nerve which I do not understand, and with which I am not quite satisfied. I shall enter a little more into this, and you will be better prepared for the inquiry, when the muscles of the eyeball come in order before us.

The Abducentes, or Sixth Pair of Nerves.—These nerves arise from a transverse sulcus on the medulla oblongata, between the pons varolii and the proper motor origin of the 7th pair, and they are clearly referable to the central columns of this prolongation of the spinal chord. Therefore we find them on the *inferior* surface of the spinal chord, and springing from its *central* column; and when we look more attentively at it, we can trace *the fibriculi springing in a line* from which it derives its origin, and uniting to form it. We trace it to the cavernous sinus, and through the foramen lacerum into the orbit, where it divides, and some branches of it are distributed over the retractor muscle, a muscle peculiar to quadrupeds, answering the purpose of hands in withdrawing the eye from danger, if that danger cannot be warded off, and the main bulk of the nerve going on to the rectus externus or the abductor of the eye, by means of which the eye rotates outwardly, and which is also a kind of antagonist to the rectus internus, and the superior oblique. Then I can clearly understand that this is *a nerve of voluntary motion.*

Its Anastomoses with the Great Organic Nerve.—There is a peculiarity about it, however, which I do not understand. There are *some minute anastomoses with the other motor nerves of the eye*, and with the ophthalmic branch; that I can comprehend; but there is a branch, a very considerable branch, given to each of the abducentes from the *great organic nerve*. They evidently go not from the sixth to the organic, but they come up in bulk through the base of the skull, and unite with the sixth. I shall, by-and-by, be assured, and I shall understand the reason of it, that this organic nerve freely anastomoses with every other nerve and every part, but the reason of these considerable branches being sent to the sixth, is not yet apparent to us. For the present I leave this part of our subject, again reminding you, however, that this abducens is a kind of antagonist of the superior oblique, the character of whose nerve a little puzzled us. If it should prove to be an organic nerve, why here is plain and palpable union with the great organic to correspond with it.

The Linguales, or Twelfth Pair of Nerves.—Still proceeding posteriorly, and in the same track, we find the lingualis, the last of the simple nerves of the cranium, arising also from the *inferior* surface of the medulla oblongata—arising also, but not so decidedly, from the *central* column, and *formed from fibriculi arising in a line.* And here I begin to have a clearer conception of another nervous character, of which I had not a trace in the nerves of pure sensation. This nerve is scarcely formed before it anastomoses, and that by many a filament with the cerebro-visceral—its union with the sub-occipital and the first cervical cannot escape my observation; and when I dissect it carefully, and especially its descending branches, I find it allied with the second cervical, with the spinal accessory, and with the great organic nerve; and then its superior branch unites with the gustatory branch of the fifth, in endless anastomoses, making a complete plexus, or network, of nervous filaments. I have many an important office to be performed by this little agile member the tongue, and I must call in the aid of many a neighbouring muscle, or almost all of them, in order to effect them. After all these anastomoses with motor and organic nerves, the main branch, accompanied by the fifth, penetrates into the substance of the tongue, pervades every part of it, and is lost in innumerable minute ramifications. This then is plainly *a nerve of voluntary motion.*

Characters of the Cerebral Nerves of Voluntary Motion.—And now, Gentlemen, after a somewhat long and intricate, yet I trust not quite unpleasant journey, we have arrived at a resting place. This is the last of the pure voluntary motor nerves; and, comparing them together—leaving out at present the fourth, with which I have said I scarcely know what to do—we obtain these characters of the cerebral nerves of voluntary motion, which may guide us materially in our future inquiries. They arise from the *inferior surface of the prolongation of the spinal chord, and from its central column, by numerous fibriculi attached in a line to the surface of the brain, and they anastamose freely with neighbouring nerves.*

TEMPERATURE OF WHITES AND NEGROES.

In a manuscript memoir on his voyage to Central Africa, presented to the Academy of Sciences by M. Douville, he has mentioned some experiments made in Africa on the difference which exists between the temperature of these two races, according to age, sex, &c. It results from the researches of M. Douville, that the temperature of the negro is *cæt. par.* much superior to that of the white;—that the heat of negresses is greater than that of negroes up to the fifteenth year of their age, but after that period less, but still greater than that of whites;—that the negroes diminish in temperature as they grow old; and, finally, that the old negroes have still a higher temperature than the whites.

ON THE IMPORTANCE OF

HYGROMETRICAL OBSERVATIONS

IN METEOROLOGY.

Read before the Franklin Institute of Philadelphia.

By JAMES ESPY, *Esq.*

THE importance of hygrometric observations, as forming a part of a meteorological journal, especially as we are now furnished with a hygrometer, which will inform with certainty, not only of the relative quantity of vapour in the atmosphere, but of the absolute quantity, induces me to offer a few remarks on this subject to the members of the Franklin Institute, for their consideration.

The object I have in view is, to induce scientific bodies to use their influence in having meteorological journals kept in different parts of the country, on the same plan, and to be published monthly. Many persons, for their own private gratification, are now keeping imperfect meteorological journals, who would feel a tenfold interest in the subject if they knew how to keep a more perfect journal, with no more trouble, and at the same time contribute greatly to the public good. As many persons in the country may be willing to keep a journal who are not furnished with a Daniell's or Jones's hygrometer, it may be useful to know how its place may be supplied. Any contrivance to obtain water in a common tumbler, cold enough to cause moisture to settle on the outside, will answer the purpose. During the summer months, I have generally found pump-water sufficiently cold, and in other months, I have been obliged to resort to salt and snow. Any of these methods I consider preferable, both in simplicity and accuracy, to Daniell's or Jones's, for by mingling waters of different temperatures, one above the other below the dew-point, the exact point may be obtained to great nicety in a short time. Whereas in using the evaporation of ether, the temperature is suddenly depressed below the dew-point, sometimes to a considerable distance, and it requires much more time and skill to arrive at even tolerable accuracy.

However, when I could not procure ice or snow, I attached a small piece of muslin, or a thin piece of sponge, to one side of the bulb of a common thermometer, and poured a small quantity of ether upon it, carefully watching the naked side, to see when the dew began to settle on it. When it settled very rapidly, the thermometer had sunk below the dew-point, but by wiping off the dew, and letting it settle again repeatedly, I could obtain a pretty accurate result. I have frequently compared these results with those I obtained by means of the cold water in the tumbler, and the agreement was very satisfactory.

In taking the temperature of the air, also, great care is required, or errors will be committed. If the thermometer is surrounded by bodies, either hotter or colder than the air, it will be affected by radiation; in the former case, it will be a temperature too high; in the latter too low. In a clear calm evening, after sunset, I have known a thermometer placed near the ground, and exposed to the open sky, to indicate from eight to twelve degrees below the temperature of the air, which may be obtained nearly by swinging the thermometer rapidly round. So during the day, if it is exposed to bodies heated by the sun, it will sometimes be several degrees warmer than the surrounding atmosphere, which is not heated by radiation, but only by contact with solid bodies.

In cloudy weather, however, no such effects take place, for then the surface of the earth, and all other solid bodies, have their radiated caloric returned by the clouds, and consequently retain the temperature of the air. On cloudy nights the surface of the earth does not cool below the *dew-point*; and, therefore, there is no dew. Even in clear nights, when the dew-point is far below the temperature of the air in the day, the surface of the ground does not sink in temperature to the dew-point by radiation, and in such case no dew is formed. I have observed in August, when the dew-point has been as high as 76° Fahr., when the temperature of the air during the day was above 90°, that but little dew was formed during the night in the city of Philadelphia. No doubt more was formed in the country, where the surface of the ground was not exposed to radiation from hot brick walls. Be that as it may, I never saw dew begin to form in the grass in my yard, until its temperature had sunk to the dew-point. Indeed no reason can be imagined why it should settle on the grass before it cooled by radiation to the dew-point, any more than in a tumbler of water above the dew-point. The phenomena are the same. The dew does not, in either case, fall through the air; but the vapour which is in the air, equally diffused by its own elasticity, when it comes in contact with solid bodies, cooled by radiation or otherwise, below the dew-point, is deprived of so much of its caloric, that it is no longer able to maintain its elastic state, and is precipitated in the form of dew.

An objection may be urged against this theory, which at first appears plausible. It

is known that dew does not appear under a shed, or even under a covering of thin muslin, spread out at a distance of some feet from the ground; and, therefore, it is inferred that dew falls through the air from the upper regions. The true reason, however, is, that the temperature of the grass or other bodies under the cover does not fall to the dew-point, the radiations being returned by the cover, which may easily be ascertained by placing one thermometer under the cover where there is no dew forming, and another in the grass beyond the cover where the dew is forming, when the difference will be detected. I have seen a difference of eight or ten degrees, under the protection of a silk handkerchief, even when the air had a free passage between it and the ground.

Hence is demonstrated the great utility of even thin coverings over tender plants during clear nights in the spring, when the surface of the ground is likely to be cooled by radiation down to the freezing point. Plants so protected may be preserved, while those in their unprotected state will be entirely destroyed.

Another argument against the theory of dew here proposed is, that the lower parts of the atmosphere do not contain vapour enough to form such heavy dews as sometimes appear, when more vapour is precipitated than is contained in a hundred feet of the atmosphere, in height, and it cannot be presumed that all this air comes in contact with the surface of the earth during a calm night, when the dew is the greatest.

But this objection will have no weight, when we recollect what Dalton, I believe, first demonstrated—that aqueous vapour is not supported by the air, but by itself; or, in other words, if the air should be suddenly annihilated, the aqueous vapour which is diffused through it by its own elasticity would neither sink upon itself, nor expand to a higher elevation; but its own elasticity would just be able to support its weight without further condensation. If now we suppose the lowest stratum of this vaporic atmosphere to be changed into dew, it is manifest the whole vaporic atmosphere must descend; and this takes place whether we suppose an atmosphere of common air or not: a longer time will be required to settle through the air than if there was none; but the ultimate effect is the same in both cases.

Indeed the hygrometer itself affords a most satisfactory proof that aqueous vapour is not supported by atmospheric air; for it is no uncommon thing for the dew-point to remain the same through the night, indicating that the density of the vapour continues unchanged, which could not be the case if vapour were supported by the air. For as vapour is about one-half the density of air, even when at its greatest density, unless compressed by art, it would rise rapidly through the air, from its levity, and leave the lower parts absolutely dry during the night, whilst no vapour is sent up from the ground to take the place of that which, by supposition, has ascended into the upper regions of the air. Surely it will not be said that vapour is sent up from the ground at the very moment dew is deposited there.

It must not, however, be supposed, that after vapour is condensed into cloud, it continues to press only on the vapour below it. Part of its weight is then supported by the air, and the vapour below being relieved from part of its pressure, immediately upon the formation of the cloud, expands, and the dew-point falls, as will be shown hereafter.

It is well known that the increase of aqueous vapour in the atmosphere over a particular region, will depress the barometer, because this vapour is only about one-half the specific gravity of common air; but there are so many causes which produce a depression of the barometer, besides the rise of the dew-point, or increase of moisture in the air, that a fall in the thermometer is not an infallible sign of rain; yet I have no doubt that these two instruments united, will enable the meteorologist to predict rain with absolute certainty, after a sufficient number of observations have been made to eliminate by induction a general law.

In the course of these observations, no doubt many curious facts will be discovered which the present state of science does not enable the philosopher to predict.

I will mention one here, which was so contrary to my anticipations at the time I first observed it, that for a while I doubted the possibility of ever arriving at any useful knowledge on the subject.

In the summer of 1829 there had been a very long period of dry weather, and I had been for many days taking the dew-point very frequently, with much anxiety to discover the first indications of rain, when suddenly all my hopes were extinguished, by a fall in the dew-point of several degrees in the course of half an hour, but to my great astonishment, in about two hours and a half, there came on one of the most violent showers I ever witnessed. For several days previous to this, the barometer had been falling, and the dew-point rising, (showing that the watery vapour was increasing), whilst the temperature of the weather had remained nearly stationary. I had been very particular in taking the dew-point, so that I felt pretty confident no

error was to be attributed to the observation; but still I was anxious to have an opportunity of witnessing a similar result.

Nothing similar was observed, though it might frequently have occurred, until the 16th July 1830, when I witnessed the following remarkable phenomenon. The weather had been very warm and dry for some days, the hygrometer being nearly stationary at 74°, for I had taken the dew-point very frequently, and on the day of the present occurrence I was sitting with the means of taking the dew-point with me in my yard, when I perceived it suddenly sink three degrees in a few minutes, and, at the same time, I observed a cloud forming immediately over my head, and, in a few minutes more, the whole time, not more than half an hour, a gentle shower of rain succeeded, hardly enough to wet the pavement. Immediately after the shower the dew-point rose again to its former elevation 74°. Not long after this I had the satisfaction of recording an observation of the same phenomenon, which renders it absolutely certain that sometimes, at least, the dew-point sinks rapidly, and to a great extent, while it is raining in the neighbourhood. From July the 16th to the 19th, the dew-point gradually rose from 74° to 76°; it then remained stationary at that point night and day, the mean temperature of the air being about 90°, till the 22nd, at five o'clock p. m. At five the dew-point was 76°, as it had been for four days before. At twenty minutes after five, the dew-point was 70°; at thirty minutes after five it was 76°; and at six o'clock it had fallen to 62°, that is 14 degrees in one hour.

During all this time there was a great appearance of rain in the north—vivid lightning, thunder audible, and a strong wind blowing towards the cloud; and the temperature sunk twelve degrees. At forty-five minutes after six, the wind had nearly ceased; but it did not begin to rain till nine, and rained but very little; but, as I understood the next day, it had rained very hard between five and six within a few miles of the city; the barometer sunk $\frac{1}{100}$ of an inch, while the wind was blowing; but rose again when it became calm. The next morning the dew-point had risen nine degrees at six o'clock, and at two p. m. one degree more, being then within four degrees of its previous state.

It would not readily occur to any philosopher, a priori, that the dew-point would fall just previous to the commencement of rain, or while it is raining in the neighbourhood; but after the fact is known, it is easily explained, on the principle first demonstrated, I believe, by Dalton, and afterwards discussed at large by Daniell—that aqueous vapour forms an atmosphere independent of the atmospheric air, and that these two fluids press only on particles of their own kind, and not on each other, consequently as soon as it begins to rain over a small portion of country, as it often does in a summer shower, the equilibrium of the vaporic atmosphere is disturbed, a partial vacuum of vapour is formed over the region where it is raining, and the vapour from the surrounding regions rushes towards the cloud when the pressure is less, diminishing the tension of the vapour in the regions around the cloud, and thus lowering the dew-point. How so rapid a depression of the thermometer took place, I am not prepared to explain, but the wind towards the cloud was no doubt produced by the impulse of the aqueous vapour against the particles of atmospheric air.

The only objection occurring to me which can be brought against this explanation, is, that the wind is known to blow in all directions from a falling shower, just at the borders of the shower. But this does not invalidate the explanation given above, for this effect is produced by the pressure of the drops of rain, which may be considered as exerting their whole force of gravity on the air beneath, as soon as they cease to accelerate in their descent towards the surface of the earth, but the wind thus produced is known to extend not far beyond the skirts of the shower, and, of course, at the distance of two or three miles, it may yield to the more extensive influence of the cause assigned above.

My object, however, at present being not to assign causes for phenomenon, but to cause them to be observed and recorded, I proceed to another subject, which I hope will engage the particular attention of all those who may hereafter keep meteorological journals. That is the evaporating point. By the evaporating point, I mean the point to which the thermometer will sink by being surrounded by wet paper.

I some time ago instituted a course of observations on this subject, constantly comparing the evaporating point with the dew-point, principally with a view to ascertain if any law could be established by which the dew-point could be known, by having the distance of the evaporating point below the temperature of the air. In the first part of these experiments I took it for granted, as stated by Mr. Leslie, that the evaporating point is the same, whether the thermometer is at rest in still air, or moved with rapid motion; but I have ascertained by not less than fifty observations, made with a view to decide this one point, that this is not the fact; for the thermometer covered with wet paper invariably stands lower when moved rapidly, than when per-

mitted to remain at rest in still air, sometimes to the amount of four degrees, and this too when the surrounding solid substances are known to be of the same temperature as the air, so that the thermometer could not be affected by radiation. The reason assigned by Mr. Leslie is so plausible, that nothing but experiment could have led me to suspect its fallacy. He says that the motion produces a more rapid evaporation, and of course more cold; yet the latter effect is exactly neutralized by the warm air coming in contact with the thermometer. I have not yet been able to ascertain the law desired; but that such a law exists I have no doubt; and if it could be ascertained, the trouble of ascertaining the dew-point would be entirely avoided; for the evaporating point can always be obtained with as much ease as the temperature of the air. The simplest method which I have been able to think of, to obtain the evaporating point, is to take an earthen pot unglazed, which I cause to be made for the purpose, and keep it filled with water, either when the air is still, as in a room, or where it is in motion, as you choose. The surface of the pot being constantly wet, the water within will always give you the evaporating point at any moment you may wish to make your observation. This much my observations will warrant me to say on this subject, that in high temperatures in summer, the evaporating-point is nearer the dew-point than the temperature of the air; and in low temperatures in winter, it is nearer to the temperature of the air than to the dew-point, consequently there is some intermediate point where it stands half way between those two temperatures. Perhaps, also, the ratio may depend on the distance of the dew-point from the temperature of the air. It would appear that when the dew-point is very far below the temperature of the air, the evaporating point recedes from the temperature of the air, and approaches the dew-point.

By using the earthen pot to obtain the evaporating point, another very important inquiry may be answered, without any other trouble than measuring the water used in replenishing the pot; i. e., How much water is evaporated from a humid surface in a given time?

Another subject which is at this moment exciting considerable interest among the philosophers of Europe, is to ascertain the difference of temperature with shade and with sun, both with the bulb of the thermometer naked, and with one covered with black silk. Mr. Daniell thinks there is much greater difference in high latitudes than in low. In Philadelphia I have frequently found the naked thermometer rise forty degrees, and once fifty-six, above the temperature of the air, while the one covered with black was generally twelve degrees higher. Mr. Daniel also thinks, that in high latitudes, radiation after sun-set cools the surface of the ground much more than in low latitudes. In this latitude I have seldom found the temperature of the grass more than eight degrees below the temperature of the air. Whereas Mr. Wells, who wrote the beautiful little essay on dew, says he has seen it as low as sixteen degrees below the temperature of the air. This would allow frost to occur in low situations, when the temperature of the air during the night would not be below forty-seven degrees. Whereas here, frost can hardly occur, unless the temperature of the air falls below forty.

Captain Scoresby mentions, as a strange and unaccountable phenomenon, that he has frequently observed, when sailing in high latitudes, the surface of the sea begin to be covered with thin ice, immediately after sunset, while the temperature of the air was still several degrees above the freezing point. When he called it unaccountable, he probably did not think of the principle of radiation. In Peru, where they have such heavy dews every night, the power of radiation must be very great, or if not, the dew-point must be but little below the temperature of the air during the day. Some traveller, I do not recollect his name, says he has seen frost in Africa in the torrid zone. Now I do not think this at all incredible; for the temperature in those regions must fall as far below the *mean* in the night, as it rises above the mean in the day. The mean temperature of the torrid zone is about 84°, and as loose dry sand is a very bad conductor, and a very good absorber and radiator of caloric, the surface of those wide-extended deserts of sand in calm weather, may easily be conceived to become fifty or sixty degrees above the mean during the day, and fall as far below it during the night, which would bring it down to the freezing point.

The time of taking the hygrometric observations is not important; it is not so, however, with the barometer. This instrument has four diurnal fluctuations: just before sun-rise, when the air is the coldest, and about two o'clock, when the air is warmest, and of course neither contracting nor expanding by change of temperature, the barometer stands at its mean height; but sometimes between sun-rise and the hottest part of the day, when the air is expanding most rapidly by increasing heat, the barometer will stand at its greatest diurnal elevation, and some time after the hottest part of the day, when the air is suffering the most rapid contraction

from cooling, the barometer will stand at its greatest diurnal depression. In the torrid zone, the point of greatest elevation is found by observation to be between nine or ten a.m., and that of greatest depression between four and six p.m. Now as it would be very desirable that two observations should be taken every day, I would earnestly recommend that between nine and ten, and five and six in the afternoon, be the time of taking observations—and if any observer wishes to take a third observation, or if any should be unwilling to take more than one per day, let two o'clock p.m. be the hour.

From the following table it will appear what proportion of the atmosphere in weight is vapour, when the dew-point is at the different temperatures from 32° to 80°. For example, when the dew-point is at 32, the pressure of the vapour alone is $\frac{2}{10}$ths of an inch of mercury, that is, $\frac{1}{130}$th of the whole, if the barometer stands at 30 inches; and if the dew-point should rise to 80° Fahr., which I believe it never does, for I have never seen it above 76°, then would the pressure of the vapour alone be equal to one inch of mercury, that is, one-thirtieth of the whole atmospheric pressure.

TABLE.*

Temp. of Dew-point.	Weight of Vapour, Inches.	Temp. of Dew-point.	Weight of Vapour, Inches.
32	0.200	57	0.474
33	.207	58	.490
34	.214	59	.507
53	.221	60	.524
36	.229	61	.542
37	.237	62	.560
38	.245	63	.578
39	.254	64	.597
40	.263	65	.616
41	.273	66	.636
42	.283	67	.655
43	.294	68	.676
44	.305	69	.698
45	.316	70	.721
46	.328	71	.745
47	.339	72	.770
48	.351	73	.796
49	.363	74	.823
50	.375	75	.854
51	.388	76	.880
52	.401	77	.910
53	.415	87	.940
54	.429	79	.971
55	.443	80	.1000
56	.458		

* Vide Turner.

TREATMENT OF THE

MALIGNANT CHOLERA

ON BOARD THE

CUMBERLAND, CONVICT HULK.

Efficacy of Mustard Emetics—Failure of the Saline Treatment and Venous Injections—Evidences of Contagion.

To the Editor of THE LANCET.

SIR,—The malignant cholera attacked the convicts confined on board the *Cumberland* convict hulk on Sunday the 10th ult. As in most other instances, the first seizures were the most malignant; every case for the first ten days was severe: they then assumed a less violent form, and by the 27th it ceased to attack them. During that period 198 patients were under treatment, of whom 33 died, including one petty officer, and one nurse, and not including the case of my friend Mr. Conway, surgeon of the Ordinary, who caught the disease while zealously and faithfully assisting me in my duties. He was struck with collapse at once, and never rallied. Instead of entering into an account of symptoms and cases, I will merely state, that in the admirable details of Mr. Scott, in his reports, are to be found the exact picture of the disease, as it happened in this hospital. The symptoms we observed show the fidelity with which he has pourtrayed the disease. In the general treatment of the complaint, I have to state, that when the men were attacked, in the first instance, with severe purging and vomiting, accompanied by prostration of strength, and a sense of sinking at the pit of the stomach, the mustard emetic was invariably administered. It is impossible to speak too highly of its value; immediate, copious, and powerful, vomiting follows its exhibition; the blood is driven to the surface, the pulse is raised, the skin becomes warm, and a speedy expression of relief is uttered by the patient. Sixty-nine prisoners required no other remedy. Several had it repeated two or three times; one, an old man (82), had it given six times in the 24 hours, and recovered. Since the disappearance of the cholera, we have had several cases of simple diarrhœa in which it has also been given with the same beneficial result. In some cases of protracted recovery it was prescribed in drachm doses at intervals of two or three hours; it was there also advantageous.

When the vomiting, purging, prostration of strength, and sense of sinking, were accompanied by spasms, bleeding followed

the exhibition of the emetic, and was repeated, if it did not arrest the symptoms; in the early stage there is seldom any difficulty of procuring blood, particularly if attempted after the emetic. I never saw the least injury result from its employment; on the contrary, to its effects, preceded by the mustard emetic, I attribute the few cases of death, 19 only having fallen a sacrifice, who were received from the *Cumberland* between the 10th and 27th. The other 14 cases of death were patients who were in the hospital at the period when the disease broke out, and who were infected by the cholera patients. Of these, one whose thigh had been amputated a few days before, one with jaundice, two with fever, one with a severe contusion on the abdomen, two under salivation, three under phthisis pulmonalis, and four old men, were struck at once with constant purging, that is, an uninterrupted flow of serous fluid from the rectum, and complete collapse; it was evident that no treatment could avail in these instances. When the disease continued in spite of the V. S. and mustard, calomel in 10 or 20 grain doses, in combination with camphor or capsicum, ammonia, ether, and mustard cataplasm, were chiefly relied upon. The acetate of morphine was found decidedly superior to opium in substance or tincture. The coldest water which could be procured was the only drink permitted, and though we have numerous high anathemas against its use, I venture to assert that it is the best which can be given, and that it is the fluid of all others most liked by the patient.

The saline treatment recommended so strongly by Dr. Stevens and his advocates was fairly and fully tried. It was on the faith of their statements that I ventured to deviate from the above plan. Their cases and mine, however, must have widely differed in their nature, for assuredly never was mode of treatment more unsuccessful. I deeply lament ever having trusted to it.

The favourable manner in whic the saline injections have been introduced to our notice, could not fail to demand our confidence. You will see in a letter from Dr. Anderson, surgeon to the *Aboukir*, extra Cholera Convict Hospital Ship at Chatham, to the Central Board of Health, in THE LANCET of the 23d ult., that he speaks with exultation of the success of the practice, and the letter is actually headed, "Injection of saline fluids into the veins. Five cases, three of which were successfully treated." If any one has been induced to try the practice on my friend's hurried statement, it would be a matter of regret, for those whom he thought convalescent have since been numbered with the dead. The proportions of the fluid injected, the temperature, and the method employed, were such as have been recommended by Dr. Latta. The phenomena attendant on the injection were, an augmentation of temperature, a gradual disappearance of the blue colour, a restoration of arterial action, an increase of respiration, and a general feeling of comfort by the sufferer. In one case, sleep took place during the process. All these effects were, however, transient—in a very short time rigors supervened, and the train of symptoms which had been averted reappeared, and required the repetition of the injection, or the aid of other remedies. While the fluids were passing by the rectum it seemed to do no injury, but when this was not the case, the injection required to be very slowly introduced, the organs of respiration, and, eventually, the brain, being evidently affected by it. Death ensued in every case except one, who was attacked by phlebitis in a most aggravated form, and now lies in a precarious state, and has one large abscess on the shoulder, and another on the hip. The following table shows the general result.

Cases of Cholera in which Saline Injections were employed.

Name.	Stage of Disease.	Quantity injected.	No. of times injected.	Event.
Thompson ..	Second	72 ounces	Once	Died 1¼ hour after injection.
Healy	Third	144 ditto	Thrice	Ditto 56 hours
Rowlands ..	Ditto	304 ditto	4 times	Ditto 8 hours
Birch	Ditto	96 ditto	Twice	Ditto 4 hours
Johnson	Ditto	216 ditto	Twice	Ditto 13½ hours
Eccles	Second	96 ditto	Once	Ditto 9 days
Higgens	Third	96 ditto	Twice	Ditto ½ hour
Longbottom..	Ditto	80 ditto	Twice	Ditto 2 days
Cooper.....	Second	192 ditto	Twice	Alive.

In the cases of Johnson, Rowland, Healy, and Birch, the insensible draining had begun before the injection was attempted, but not so in the others. Eccles, who lived nine days after the process, was incessantly restless, and exhibited well-marked symptoms of cerebral irritation, but decidedly different from the affection of the head met with in the consecutive fever.

It is difficult to account for the appearance of malignant cholera on board the *Cumberland*. Only one case had previously occurred at Rochester, and two at Chatham; but the period of their occurrence is too remote to authorise the suspicion that they had any connexion with the irruption of the disease on the 10th of June. A boy, however, was some days previously attacked with cholera on board a barge at a short distance from the hospital ship, and brought on board; he had medicine administered, was carried to Gillingham, and died. At the same time, at a short distance from the *Cumberland*, the *Brothers*, a timber ship, was in a most unhealthy state, having discharged several of her labourers on account of bowel complaints—one or two of which afterwards terminated in most malignant cholera. One of their men was brought to us by the local board of health on the 23d, who had been ill for several days; he was in the collapsed stage, but recovered. We received a second from her, but he died immediately after his admission. In the absence however of any positive proof of its introduction by contagion, there is abundant evidence to prove its contagious character when once introduced. It attacked equally all the decks of the *Cumberland*, all the hospital nurses with two exceptions, some of the invalids on the Invalid Hospital Deck, 28 of the prisoners previously in the hospital with other diseases, one of the guards, and two of the medical officers, and several of the others had severe premonitory diarrhœa.

The most decidedly marked evidence of contagion which I have met with in this neighbourhood, was the introduction of the disease into the village of Lower Upnor. One of the labourers on board the *Brothers* obtained for his mother (Mrs. Collins) the washing of the linen of the crew, which he carried backwards and forwards as he went to and from his labour. She caught the disease and died; several persons in communication with her met a similar fate, and the complaint became general in the village. The number of prisoners on board the *Cumberland* and *Canada* at the period of invasion, was between seven and eight hundred. The sufferers from cholera were therefore few, and the small amount should be attributed to the constant inspection kept up by the officers, and the care they took to report every case of bowel complaint immediately on its occurrence. They were unfortunately cut off from their usual labour, and it was even gravely recommended to send the hulk to Standgate Creek. In such weather, and in such a crowded condition, many of the prisoners labouring under strong depressing passions, this measure must have proved a very fatal one. The public mind, however, was systematically worked up to this point; so much so, that my own appearance on shore, after four days and four nights incessant labour in the sick wards, was looked on with as much horror as if a mad dog had been in the street. They managed things more wisely at Deptford and Woolwich, where the existence of the disease among the prisoners made no interruption to their usual labour in the Arsenal and Dock-yard; they infected no artillery man, marine, or civilian, proving, that though contagious, the contagion is, like all other contagion, communicable only in a very low degree *when freely diluted*. It is confinement under exposure to the atmosphere of many cholera patients, which forms the greatest danger.

In forwarding this sketch, I beg to conclude with returning my grateful thanks to Dr. Anderson and Mr. Whitmarsh; also to the naval and civil assistants, whose thirst for knowledge expelled every idea of personal danger. To those officers in the naval medical department who frequently favoured me with their advice, my obligations can never be repaid. Poor Conway, alas! was cut short in the midst of his active exertions to investigate the nature of this singular disease, while on a branch of public service to which he did not belong; his death was as much a sacrifice as ever was that of the most desperate "forlorn hope" in battle. I am, Sir, your obedient servant,

ARCH. ROBERTSON, Surg.

Rochester, July 19.

TREATMENT OF THE

MALIGNANT CHOLERA

AT THE CHOLERA HOSPITAL, WESTMINSTER.

THE house fitted up as a Cholera Hospital for the united parishes of St. Margaret and St. John, Westminster, having been placed under the care of Mr. Furnivall, with Mr. Ollier as the consulting surgeon, the cases (treated conjointly there and at their own homes) now exceeding 100, I, as the partner of the first-named gentleman, having been actively engaged in attendance upon the whole of the above, have (with their permission) compiled a few facts re-

lating to the disease and its treatment, which I trust will be found interesting.

With the above object I have taken the first 100 cases, and have arranged them into a tabular form, so as to show the proportion of deaths and recoveries, as modified by age and sex, and the treatment having been conducted at their own homes, or at the hospital.

At the first view it will be seen that the advantage is greatly on the side of youth, the male sex, and treatment at an hospital.

One Hundred first Cases.	Number of Patients.	Deaths.	Recoveries.	Total.
Number of Males	46	32	14	46
Number of Females	54	41	13	54
		73	27	100
Number treated at their own homes	34	27	7	34
Number treated at the Hospital	66	46	20	66
		73	27	100
Number of patients under the age of 15 years	13	9	4	13
Ditto between 15 and 30	13	5	8	13
Ditto between 30 and 50	32	21	11	32
Ditto between 50 and 70	37	33	4	37
Above the age of 70	5	5	0	5
		73	27	100

With regard to the number of deaths, it should be observed, that no cases have been returned as cholera, unless the evacuations unequivocally presented the true characteristics of that disease. Also it has been very rare for any of us to see the case before it had been far advanced in the collapsed stage. Many have been for some hours previously under the treatment of others, and have been sent to the hospital in a dying state; some have actually expired on the road. Again, when it is noted (as the table shows) that one-half the deaths have taken place in persons exceeding fifty years of age, the recoveries above that period amounting only to four, I think our exertions have not gone altogether unrewarded.

There have been few exceptions to the fact, that a premonitory diarrhœa of from some hours to three days duration takes place. We have never failed to check the diarrhœa, and our cases have been numerous, and many of them severe. From the tendency of the true choleric attack to take place during the night, an opiate at bed-time is always requisite when the diarrhœa is urgent.

It is by no means my intention to give any detailed plan of treatment, but a few observations on some of the principal points seem necessary.

The hot-air bath, tins, or bottles of hot water, are always desirable; as an external stimulant the mustard cataplasm is the most efficacious. With regard to internal stimulants, where there exists much irritability of stomach they invariably increase it; few patients will continue for any length of time to take brandy-and-water as common drink, even when cold and much diluted.

To allay the excessive thirst, any fluid may be allowed, in a quantity not exceeding a tablespoonful at one time, a large quantity generally exciting vomiting. In some cases great benefit in this respect was derived from frequently sponging the palm of the hands with cold vinegar.

Bleeding in the stage of collapse was positively injurious. In the early stages, and after reaction has commenced, it may be used with much benefit, but requires judgment. Injections into the bowels have never been omitted, warm water alone has been found beneficial, it may be rendered more stimulating by the addition of brandy and a little common salt; if the cramps are violent, the addition of a drachm of laudanum rarely fails to relieve them, tincture of valerian or assafœtida may be conjoined. The stimulating enema should be repeated at intervals.

We have found by far the greatest benefit to result from moderate but frequently-repeated doses of calomel, or blue pill and opium; we generally prefer the latter combination, the interval between the doses being gradually lengthened, but persevered in at least until the evacuations contain feculent matter. Care should be taken that very little fluid be given for some

minutes after the pill is swallowed, or its rejection may be the consequence. When reaction has been established, it must be moderated and treated according to circumstances and the judgment of the medical attendant, but officious interference is to be avoided.

I am afraid that theory, hypothesis, and empiricism, have too far superseded science and judgment in the treatment of this disease. The saline mode of treatment was most assiduously tried in several cases, but I regret to say the result has not been so favourable in our hands as stated to have been in those of others.

In four cases the injection of saline fluids into the veins was tried, but not with sufficient success to induce us to persevere.

Latterly, several cases, after recovering from the stage of collapse, have become comatose, with stertorous breathing, and other symptoms of effusion on the brain; they have generally terminated fatally.

J. WRIGHT, M.D.

3, Princes-Court, Storey's-Gate,
Westminster, July, 28th, 1832.

CASES OF

MALIGNANT CHOLERA,

TREATED PRINCIPALLY BY

CORROSIVE SUBLIMATE AND ACETATE OF LEAD.

By F. J. HART, *Esq., Surgeon.**

CASE 1.—James Snell, aged 33, a leather-dresser, of rather a spare habit of body, had been labouring under diarrhœa for three or four days, but on the morning of Thursday the 12th inst., was seized with violent cramps in the stomach and legs, and excessive vomiting and purging, which obliged him to apply for assistance.

At 12 o'clock I saw him. At this time there was no pulse to be felt at the wrist or ancles, the feet and hands were cold, contracted, and of a leaden hue; the eyes very much sunk and ghastly; the vomiting and purging still continued, and the cramps in every part of the body very distressing; but the skin, except the extremities, was quite warm; the pulse in the upper part of the arms and legs, and the heart, was at 90, small and hard. Ordered mustard cataplasms to the feet, mercurial friction over the abdomen, to take brandy-and-water in small quantities, and a teaspoonful of the following mixture, in brandy and-water, every quarter of an hour until the sickness ceased, with one pill every half hour.

℞ *Acid. nitrici*, gtt. xxx;
Tinct. opii, gtt. xl;
Aquæ puræ, ʒiss. *M. ft.*

℞ *Hydrarg. oxymur.* gr. iv;
Plumbi s. acetas, gr. iv;
Pulv. opii, gr. vj;
Ol. Caryoph. q. s. M. ft. pil. xii.

At 6 p.m. very little change had taken place, except that the sickness had somewhat abated, and he complained of his head being light. At 10, the sickness had entirely ceased, the purging was become less frequent, the pulse could now be felt at the wrist, though very feeble, and the cramps were wholly confined to the body; the hands and feet were regaining their natural state; he complained of great thirst, and had passed no water. To continue the pills every hour.

On the following day (Friday 15th), he was much better, had been in a violent perspiration all night, the purging had ceased, no return of sickness, the cramps much abated, the hands and feet perfectly recovered, and the pulse 120, soft and full; complains of his head, and great thirst. Ordered to drink lemonade; the abdomen to be bathed with hot water; to discontinue the cataplasms, and to take two tablespoonsful every three hours of the following mixture:—

℞ *Quininæ sulph.* gr. vj;
Acid. sulph. dil. ʒi;
Tr. card. co. ʒiij;
Aquæ ad ʒvj.

In the evening no return of the violent symptoms; head much better; perspiration still continues; pulse 100, and soft.

On Saturday he was gaining ground very fast, and on Tuesday I met him, when he only complained of great weakness.

CASE 2.—John Hubbard, aged 30, the captain of a vessel, of good health and sober habits, was seized on Tuesday, the 10th inst., with a violent purging and vomiting, which obliged him to go home. At six o'clock in the evening I saw him, when the vomiting and purging had increased, the pulse hard, small, and feeble, cramps in the legs and stomach, great prostration; still, both the hands and feet were warm, and the eyes were not sunk; ordered warm bottles to the stomach and legs, to take nothing but a little brandy and water, or gruel with brandy in it, and one of the pills every half hour. At nine o'clock he was much altered for the worse, the pulse quite gone at the wrist, hands and feet cold and blue, the eyes sunk, and countenance ghastly, great oppression at the chest, and the voice nearly gone; the cramps were so severe that the poor fellow writhed from one side of the bed to the other with agony, the sickness and purging occurring every

* Communicated by the Central Board of Health.

five minutes. I considered the case hopeless, and ordered him a teaspoonful of the mixture in brandy-and-water every five minutes until the sickness ceased, and then to take, in addition to one of the pills as above, one containing two grains of P. *Opii* every half hour, until the violence of the symptoms was abated. I saw him in the morning, and never did I see so great a change produced in so short a time; the sickness had ceased in half an hour, and the purging about four o'clock; the cramps had entirely subsided; the feet and hands were quite warm and natural, pulse very full, 120, and hard; the tongue from white had become of a vivid red, and the difficulty of breathing was gone; but his head became exceedingly light. I bled him ad ʒxx, ordered to continue the mixture every hour, and drink lemonade. In the evening every unpleasant symptom had subsided; he had passed water freely, and been in a violent perspiration all day; his mouth rather sore, and complained of weakness. Ordered the quinine mixture, a repetition of which completed his cure.

OBSERVATIONS
ON
MERCURIAL FRICTIONS
IN
MALIGNANT CHOLERA.

By J. Tytler, *Esq., Surgeon, Bengal Service.*

(*Abstracted from the "Transactions of the Medical and Physical Society of Calcutta," vol.* 5, 1831.)

Of the innumerable remedies proposed for this terrible epidemic, scarcely one is in the least to be depended on. Antiphlogistics, antispasmodics, narcotics, stimulants, cathartics, diluents, have been tried in every possible combination, and every one tried in vain.

In the midst, however, of these innumerable trials, it appears to me that one remedy has been constantly and unaccountably overlooked; although I trust, before concluding, to be able to demonstrate that its efficacy is great and unquestionable, and that it may be used with wonderful success in cases where everything else is either useless or impracticable. This remedy is mercurial friction on the skin.

Mercurial frictions do not accord very clearly in character with any of the classes of medicine above enumerated; they can scarcely be said to be antiphlogistic, antispasmodic, narcotic, stimulant, cathartic, or diluent. In what point of view, then, are they to be considered, and on what theoretical principle are we to employ them?

To me there appears but one such principle; though it is, I believe, the fashion of the present day, to neglect and to decry it; I mean Hunter's theory of the incompatibility of morbid actions. It is to be inquired, whether mercurial irritation is not also incompatible with cholera, so that, if the one be artificially excited, the other must spontaneously cease. Were we fortunate enough to be able to establish this as truth, the treatment of cholera would be most simple and satisfactory; and would consist only in exciting salivation by the shortest and least irritating means; after which, we might hope that the cure would be ensured.

The advantage of mercurial frictions over the administration of that medicine by the mouth, are great and many. By rubbing in, all irritation to the stomach is avoided. In the second place, it appears to me, that in cholera there exists a spasm of the absorbing vessels of the intestinal canal, which prevents their conveying into the system even such of the calomel as may be retained. Accordingly, it may be observed, that it is extremely difficult, if not impossible, to salivate persons under such circumstances, by any quantity of calomel whatever; while, as I trust to be able to show, no such spasm exists in the vessels of the skin, and the system is affected through them very readily. I shall proceed to the detail of those cases which have led me to the above opinions.

Case 1.—T. R. B., a country-born lad, of sickly constitution, subject to attacks of fever, and suspected to have disease of the spleen. He had eaten a very heavy dinner of fat broth the day before, and at four o'clock in the morning of the 17th February, was attacked by cholera morbus. * * * Fixing my attention on that very remarkable feature of the disease, the total absence of bile, either in the matter vomited, or in the alvine discharge, it suddenly occurred to me, that mercurial frictions might have a tendency to retard this suppressed secretion. I therefore directed a quantity of mercurial ointment to be rubbed into the thighs every two hours, and 12 grs. of calomel, and 40 drops tinct. opii, to be given at the same intervals. I visited him twice more in the course of the day, and found that he vomited every dose of calomel as fast as he took it, and that the purging went on uninterruptedly; in short, there was little appearance of amendment, and on leaving him, I felt fully persuaded that

next morning would give me notice of his death. To my great surprise I was informed next morning, that he was considerably better, and on going to him, found almost every symptom of cholera removed. The vomiting had ceased, but he had had one or two watery stools. The skin and pulse were natural, and he was able to take a little light food. He was gently salivated. It was unnecessary to do more than give a dose or two of purging medicine till his mouth got well.

Remarks.—If it be here said that calomel and opium, and not the frictions, cured the disease, let it be remembered, that each dose was vomited almost as fast as it was taken: the cure evidently depended on the salivation, and the salivation surely was not produced by the rejected calomel. We may observe, too, that the cure was not effected by a restoration of the secretion of bile, as I had expected; for the watery stools continued next morning, when all other symptoms were relieved. It seems to have been simply the irritation producing ptyalism that removed the cholera.

Case 2.—About a year ago, a country-born lad at Monghyr, of almost the same age and constitution, was taken ill early in the morning, in consequence of having eaten some bazaar sweetmeats the night before. I saw him about eight a.m. Calomel, laudanum, and stimulants, were administered the whole day, with the most scrupulous punctuality, but without producing the smallest change in the disease, and he died at four p.m. These two cases were as nearly parallel as can be imagined, and to what can the difference of event be reasonably ascribed, but to the difference of treatment?

Case 3.—William Holman, private of H. C. 2nd European regiment, was left here sick by a detachment proceeding up the country. On the evening of March 7th he was attacked by cholera, and continued vomiting and purging all night. He was brought to hospital next morning, and I there found him in a very advanced stage of the disease. I directed a quantity of mercurial ointment to be rubbed into his thighs directly, and 12 grs. of calomel given internally. This was done, but the calomel was immediately vomited. Shortly after, the frictions were renewed, and the calomel and opium again given, but again rejected. This treatment was continued every two hours during the whole day. In the mean time the sickness and purging continued; the cramps were frequent and severe, and to use his own expression, when they came on, his legs became as stiff as deal boards. The cold perspiration and smallness of pulse continued; the only thing he had been able to keep in his stomach was ʒii of Madeira wine, which he had taken by sips in the course of the day. In this situation I left him, after he had been rubbed in for the last time, about eight p. m., fully expecting to hear next morning that he was either dead or dying; for I can truly say that I had never known a patient recover from such desperate circumstances. My surprise and gratification were, therefore, extreme, when on visiting the hospital next morning, I found him almost quite well. His skin moderately warm and moist; pulse 105; his mouth was very sore, and he was spitting copiously. He had a few minutes before vomited a little, in consequence of drinking some cold water, but otherwise his stomach was free from disturbance. He had taken another allowance of wine, and was so well, that I was induced to order a dose of infusion of senna. This he rejected, but found no other inconvenience from it, and had three natural stools in the course of the day. No other symptoms of disease remained, except a violent salivation, from which he gradually recovered, and was discharged cured on the 22nd.

Remark.—If it be again urged, that the calomel and laudanum cured this man, I must answer, as in the last case, that these medicines were rejected as fast as they were swallowed, and that I never saw any one restored by these medicines from such a situation before.

Case 4.—Serjeant Benjamin Church, a sober man, of the European invalids, on the 13th of March, after eating his breakfast, was seized with cold shivering and great thirst. On drinking a little water, he was immediately attacked by watery vomiting and purging. What he vomited tasted intensely bitter; but the vomiting soon stopped, and was succeeded by a sense of violent heat in the stomach, and a feeling of cutting across the navel. The purging continued, and he was brought to hospital about noon, and had there, immediately, two watery stools. When I saw him, his skin was warm; pulse 120, strong and full. Very thirsty, but the moment he tastes water, is obliged to run to stool; no actual vomiting, but very squeamish. *Venesection ad* ʒ xvi. After this he became faint; thirst continued, and other symptoms much the same. Almost immediately after the bleeding, had a copious watery stool. *Calomel* gr. xii. At next visit, half-past one p. m. complaining of extreme pain of the stomach, as if cutting. Skin very warm; pulse 120; says his fingers and toes are cramped, and feels quite cold. Is very desirous of sleeping, but as soon as he begins to dose, is roused by the cramps,

and a shooting pain in the stomach. No vomiting, but one watery stool since my last visit. *Repet. inunctio mercur. Tinct. opii* gtt. l. *spt. æther.* gtt. xl. *Aq. fontan.* ℥ ss. Soon after getting these medicines he fell asleep, and continued so for a long time. On awaking, appeared greatly better. 14th, Dilucul. seen by the garrison assistant-surgeon, Dr. Smith, who reports that he appears tolerably easy. Pulse qute regular; tongue moist; complains of pain in the abdomen. He ordered *ol. ricin.* ℥i, and this operated three or four times. 15th. Pretty well, but very weak. Thinking all danger over, and wishing to give rather a placebo than an active medicine, I incautiously ordered *mist. camphor.* ℥iss; this he took about nine o'clock, and had scarcely done so, when he was obliged to run to stool, and there was attacked by cold shivering and vomiting, as before. Vomited all his breakfast, and the retching continued very severe. What he brought up was yellow and bitter. I saw him at noon, after he had had three or four watery stools: pulse quick; skin hot; a burning sensation at the stomach, and wishes much to have a fomentation to it. *Tinct. opii* gtt. l, *spt. æther. nitr.* gtt. xl. *Calomel.* gr. xii stat. *Inunctio mercurial. femorib. fomentat. cacumin. papaver. abdomin.* The mercury was rubbed in at twelve and two o'clock; at four p. m. he complained of very severe pain in the abdomen, with coldness in the hands and feet; pulse soft and slow; he begins to have a clammy perspiration. Has eaten nothing, and has had several watery stools. The mercury was again rubbed in at five and six p. m. Seen by Dr. Smith, who reports pulse small and quick; the pain at the stomach continues. He directed *tinct. opii* gtt. xl, *pergat. c. frictione.* Eight p. m., sleeping gently. 16th, a good deal salivated; vomiting and all bad symptoms ceased. Had one black stool in the night; pulse 99; skin natural. He took some breakfast, and immediately after vomited up some black stuff, which gave him immediate relief. He has been well ever since, and was discharged on the 18th, at his own request.

Remarks.—In this case it is demonstrable that neither the bleeding, nor calomel given the first day, effected anything towards a cure. The disease returned in all its violence two days after they were given. It is impossible to ascribe it to anything but the mercurial frictions too sparingly employed the first day, but used abundantly afterwards.

Case 5.—The two next cases will, I think, show the advantages of mercurial frictions over every other practice, even yet more strongly than the preceding. They both occurred on the same day.

Serjeant John Mackenzie was left here in the same party, and on the same account, as Holman. He is a man of sober habits, but of much-debilitated constitution. He had suffered much at Cheduba from fever, terminating in enlargement of the spleen. On this account he had been a considerable time in hospital here, and was but lately discharged. On the 18th of March, after breakfast, he was suddenly attacked by excruciating pain of the stomach coming on in fits, attended with severe sickness, and attempts to vomit. The substance he vomited was, by his own account, lumps of coagulated blood about half the size of his fist. On being brought to hospital about noon, I found him rolling in agony in his cot; pulse 120; excessive and unquenchable thirst. Skin hot and clammy. In my presence he retched violently, and brought up a good deal of mucus tinged with a little blood. *Venesect. ad* ℥xx *statim.* While the blood was flowing he complained excessively of sickness and thirst; drank frequently small quantities of water, and made several attempts to vomit. At last, when the bleeding was nearly finished, he suddenly became more than formerly sick, and vomited a large quantity of the watery secretion peculiar to cholera. The pain, sickness, and unquenchable thirst, continued perfectly unabated. *Calomel* gr. xx. *Tinct. opii* gtt. xl. *Spt. æther. nitr.* gtt. xl *statim, et inunctio mercurialis femoribus diligenter.* In an hour afterwards no alteration. Vomiting, purging, and pain, as before, and so restless, it is difficult for the coolies to hold and rub him. *Repeat friction, et habeat opii* gr. ii. Six p.m. has been rubbed in three or four times as his agitation would permit. Pulse 120, and small; skin cold and burning. The very acute pain of the stomach has ceased, but he now complains of pain in the region of the liver. *Repet. frictio interval. 6 hor.* Eight p.m. much better; skin warm and comfortable; has not vomited since my last visit, but he has had several stools; has drunk and retained some tea: gums a little inflamed. Nine p.m. In all respects better; vomiting quite ceased, but still purging a little; pulse 84, and regular; skin warm and natural; inclined to eat. Next morning all disease gone, except pain in the enlarged spleen. *Magnes. sulph.* ℥i. This was retained and operated well, and he was discharged cured on the 21st.

Remarks.—In this very violent case the bleeding, though apparently indicated in the strongest manner, both by the symptoms and the constitution of the patient, was so far from being advantageous, that it seemed to accelerate the progress of the malady. Only one dose of calomel was given, and that was immediately rejected.

The opium does not deserve consideration.

Case 6.—While in hospital, attending Mackenzie, my attention was suddenly called to William Bowers, a man of Herculean strength and constitution; invalided for the loss of one of his arms; of excessively drunken and disorderly habits. He had been brought to hospital on the evening of the 18th in a state of great nervous irritation from recent intoxication. No very urgent symptoms appeared till near the morning of the 18th, when, after attemping to eat some breakfast, he was attacked by severe sickness, vomiting, and purging, and all the symptoms of well-defined cholera. I found him with pulse small and quick; skin hot, and covered with perspiration. *Venesectio statim.* After about ℥xii were taken away, he became excessively faint, and begged that the bleeding might be stopped, which was done, and his arm bound up. Directed immediately *calom.* gr. xx. *Tinct. opii* gtt. xl. *Spt. æther. nitr.* gtt. xl, *et frictio mercurial. femorib.* At six p.m. has been rubbed in twice, and is rather easier. Pulse 120, quick, and small. skin warm; has had ten watery stools, and repeated vomitings since my last visit. Longs much for a little sago and wine; this was ordered for him, and the frictions directed to be continued. Eight p.m. A good deal better; the bandage accidentally loosened from his arm, and the blood burst out a second time, and he says he lost more now than before. 19th. Seven a.m. Much better; skin cool; no more vomiting, but several watery stools. Eleven a.m. From some unknown cause the symptoms of cholera re-appeared with even greater violence than before. Skin hot and covered with perspiration; pulse excessively quick; vomiting copiously, but no stools; complains of intolerable uneasiness about the abdomen, which is hard and tumid; says he feels cold, and yet is sensible he is hot; excessively restless and agitated. *Enema sodæ muriat.* ℥i. *Ol. ricin.* ℥ii. *Aq. tepid. q. s. habeat calomel.* gr. xii, *et repet. frictio mercurial.* One p.m. The injection produced these scanty stools; vomited the calomel almost immediately; is much dejected in mind, and persuaded he is dying; nausea remains, but is unable to vomit now; longs much for beer. Repeat frictions, and give him a bottle of beer, with a caution to drink but a little at a time. Three p.m. Much better in all respects; sickness less; drank a mouthful of the beer, and then was disgusted with it; feels his gums a little sore. Repeat frictions at the end of an hour. Six p.m. Still uneasy about the abdomen, but less sick; no stools. *Magnes. sulph.* ℥i. This was vomited almost as soon as taken, and a good deal of vomiting succeeded.. *Calomel* gr. xii, *et rept. frictio.* Nine p.m. Asleep; much better, and he has ever since continued to improve, with the exception of one fit of vomiting, which he had two days afterwards. This case has been the most tedious of all, owing no doubt to his previous excessive intemperance; and he is still in hospital with pain in right side, all symptoms of cholera having disappeared.

Remarks.—On this case the same observations are to be made as before. The first calomel and laudanum could have had no share in curing the disease; since, from some unknown, though probably trifling cause, it returned in all its violence the next morning. The mercurial frictions at first employed were sufficient to check, but not subdue it. On the second day they were more freely and successfully employed. The bad effects following the exhibition of the salts strengthen what was before noticed of the impropriety of employing medicines of doubtful efficacy.

General Remarks.—No time should be lost in having recourse to mercurial frictions; and they may be begun, in a severe case, by two drachms of the strong ointment on each thigh, for the space of an hour. After the interval of an hour, this may be repeated; then after an interval of two hours, and then of two hours more. After these four inunctions, the disease will probably begin to yield, and the mouth to be effected; and the practitioners must then be guided by circumstances as to the propriety of continuing, diminishing, or leaving off the frictions. In the mean time, to support the strength, a little wine may be given, from time to time. When the irritation of the stomach has subsided, and the vomiting has ceased, whether salivation has actually commenced or not, if the constitution appear to have suffered severely, and the debility be great, it may be proper to leave off mercury, and to exhibit tonics. Of these, the most convenient will probably be found to be tincture, or extract, of bark, and when procurable, sulphate of quinine.

P. S.—Since writing the above, three other cases of cholera have come under my observation. One in a bearer, the second in an Ayah, and the third in a European invalid. All of them were treated on the foregoing principles. The bearer was brought to me about twenty-four hours after the accession of the disease, and appeared almost in its last stage. He was rubbed in three or four times, after which his friends carried him away, but I have heard that he is since well. The case of the Ayah was particularly severe. She was taken ill in the middle of the night, in my own house; and next morning,

when I saw her, had all the most aggravated symptoms of the disease, and in addition, appeared in a state of delirium. Her pulse was scarcely perceptible. After four repetitions of the frictions, she also was carried to her house by her friends. Next morning the symptoms of cholera had ceased. She appeared, however, so weak, that I judged it improper to continue the frictions, and in the course of the day gave her nine grains of solution of sulphate of quinine, with three drachms of tincture of bark, and twenty grains of blue pill. This restored her very much, and in the course of next day her mouth became sore. She required nothing else but a dose of infus. sennæ. She is now nearly well, with the exception of violent rheumatic pains in her limbs. The European invalid had been drinking the day before, and was brought to hospital, suffering from the effects of intemperance. He was attacked with exquisite symptoms of cholera about an hour afterwards. He was rubbed in three times in the course of the day, and in the afternoon was much better. He then got twelve grains of calomel. Next morning, he was salivated, and is now convalescent. In all of these cases opium and calomel were given in the first instance, but in all they were immediately vomited.

IMPUNITY WITH WHICH

VEGETABLES AND FRUIT

MAY BE EATEN DURING THE PREVALENCE OF CHOLERA.*

To the Central Board of Health.

GENTLEMEN, — An impression having existed in the public mind that the eating of fruit and vegetables has tended to promote the disease now unhappily prevalent in this country, the persons whose names are undersigned, growers and producers of these commodities, beg leave most respectfully to state for your information, and that of the public in general, that among one thousand and ten persons, employed by twenty-one individuals, being a very small proportion of the number of those connected with horticultural pursuits in the vicinity of this metropolis, the average expenditure of many of the female part of whom does not exceed 1*s.* 6*d.* or 2*s.* per week, and who are engaged in the cultivation and gathering of fruit and vegetables, *no case of cholera has presented itself*, although a great number of those so employed subsist almost entirely on a fruit and vegetable diet; exposed at the same time to the various privations necessarily attached to a life of laborious exertion, and to the frequent consequences attending incautious habits.

Feeling, therefore, most acutely the injury to which the interests of the growers are exposed, as well as those of their labourers and venders, they venture to submit to you the following statement of the number of persons employed, who both are and have been entirely free from the prevailing disease, and beg leave to subscribe themselves, Gentlemen, yours most respectfully,

(Signed) S. HUTCHINS, Chairman.
24th July, 1832.

Names of Cultivators.		Persons employed.
— Hutchins	Earl's Court ..	90
J. Wilmot	Isleworth	60
H. Fitch	Fulham	70
— Stoneham	Crayford, Kent	75
R. Attwood	Kew	36
H. Knevett	Isleworth	65
C. Knevett	Acton.....	55
J. Norris	Isleworth	200
J. Jefferys	Chiswick	40
J. Chapman	Isleworth	85
J. Poupart.....	Fulham	30
S. Poupart.....	Battersea	23
H. Myers	Ealing	21
C. Allen.....	Old Brentford.	18
W. Attwood	Mortlake	20
A. Duncer	Fulham	40
— Gainsford	Acton.....	20
— Foster	Kensington ..	14
— Humphrey	Hounslow	10
W. Wright	Charlton	30
— Barnard		8
21 Growers.	Total ..	1,010

All in health but one individual at Crayford, who was unwell when the paper was signed.

It may be added, in confirmation of the above, that in the extensive parish of Fulham in particular, which is principally occupied in gardening cultivation, *not one instance of the cholera has appeared.*

* Communicated by the Central Board of Health.

LONDON HOSPITAL.

THE Election for an Assistant Physician to the London Hospital, vacant by the resignation of Dr. Macbraire, terminated on Wednesday afternoon in favour of Dr. A. Frampton, who was elected by a large majority. The Candidates were, Dr. A. Frampton, and Dr. C. J. Fox.

CALCULI IN THE BLADDER.

ON THE ADVANTAGES OF, AND OBJECTIONS TO, THE USE OF THE PERCUTEUR COURBE A MARTEAU.

With a Case illustrating its efficiency when the Prostate Gland is enlarged.

By Baron Heurteloup, *M. D.*

To the Editor of The Lancet.

Sir,—To comminute a stone in the interior of the bladder by means of a hammer, is a process sufficiently new and unlooked for, to render it difficult for some at first to believe that it can be exempt from danger; and it is a matter of great difficulty to do away with the unfavourable impressions which are mostly made by those who, without having on the subject very accurate ideas, launch out into conjecture, and reason on hypotheses instead of reasoning on facts.

Allow me, Sir, through the medium of your widely-circulated journal, to rectify some of the incorrect ideas which may have been formed respecting the *percuteur courbe à marteau*.

It has been said of this instrument, in the first place, that it is liable to break in the patient's bladder; secondly, that its beaks were liable to separate, and so prevent it from being withdrawn; thirdly, that during the percussion with the hammer the instrument might vibrate, and cause considerable pain; fourthly, that during the percussion, the fragments might fly against the bladder and injure it; fifthly, that the instrument was not well calculated to seize the stone; sixthly, that curved instruments were useless, since straight ones always sufficed; seventhly, that it could only be employed in cases of small friable calculi; and, finally, eighthly, that it produced fragments but no powder, which rendered the evacuation of the stone more difficult.

I will endeavour to answer all these objections, which, for the most part, were made by the members of the committee, chosen by the Institute of France, to examine my labours. In the numerous opportunities which these competent judges afforded me, I proved to them, *by facts*, that their objections were unfounded; and, in order to render myself clear and methodical, I shall now notice and refute them one by one.

1st. Can the instrument break in the patient's bladder?

I have placed several instruments in the *point fixé* of the *rectangular bed*, and all my efforts to break them have proved unsuccessful. When I employed a hammer of *considerable weight*, I succeeded in forcing the branches apart, but only when I placed between them some tough, unyielding substance, such as a piece of wood or steel. Whenever I acted upon a calculus, however hard, it always gave way. I asked several persons to use their utmost efforts to break an instrument; but their attempts were equally fruitless. These experiments prove, that it is not possible to leave a portion of the *percuteur courbe* in the bladder; for the most determined resolution to produce a fracture of this instrument, outside the bladder, is unavailing. This will be readily conceived, when it is known, that no part of it is tempered.

2ndly. Are the beaks liable to separate and to prevent the instrument from being withdrawn?

Without doubt—if, as I said in the former experiment, a hammer be employed not in proportion to the power of resistance of the instrument. But, as I never operate on a patient with an instrument which has not previously been submitted to *repeated trials* with a *hammer twice the weight of the one employed in the operation*, it will be admitted that a separation of the branches need not be feared, since there is not in the hand of the operator sufficient power to produce it. All will, I think, admit, that it was not possible *at once* to perceive all the properties which were required to render this a good and serviceable instrument, or to discover what was the most advantageous construction to be adopted in its numerous details; it was only by *long* and *persevering study* that such a knowledge was to be acquired, and this explains why, in one of the first attempts that I made with the *percuteur*, the branches were warped. At that time I was inexperienced,—I employed a hammer much too heavy, and the instrument itself was imperfect, and bore but little resemblance to those which I now make use of. The proof of this is, that in the presence of the committee chosen by the Academy, I comminuted hard and voluminous calculi, without the instrument yielding in the least. For upwards of a year, I have been constantly engaged in seeking to distribute the force equally and advantageously in the different parts of the *percuteur*; for this instrument being restricted to the narrow limits of three and a half or four lines, it is in this equable distribution (so difficult to obtain) that its power and perfection consist. Sixty or seventy different models have been constructed, in order to discover in which the greatest power for breaking could be united to the greatest delicacy for seizing. And it is only after making the successive alterations which these trials suggested, that the

instrument was brought to its present state of perfection.

I have studied the force of resistance of the *percuteur*, as regards its construction and the bodies to be acted upon. I have submitted it to varied trials with hammers differing in weight, lever, and form, with solid and elastic handles; and, lastly, I have examined, on real calculi, the comparative effect of slow or sharp, gentle or hard blows; and I discovered that even this part of the operation, apparently so simple, had its difficulties and secrets. To understand this better, it is only necessary to consider the ease and nicety with which a practised hand detaches and works large pieces of flint, whilst another man, howsoever clever, if without practice, fatigues himself by efforts as clumsy as they are useless, and for a long time vainly tries to execute what appeared so simple and easy. Thus in conclusion, we find that there is no danger of producing the separation of the branches of the *percuteur*: 1st, From the far greater perfection of the instrument as regards its construction. 2dly, From the incapability of the operation to cause this separation, on account of the lightness of the hammer he employs. And 3rdly, From the additional knowledge acquired of the manner of striking that is most likely to break down calculi, which, from their shape and toughness, would perhaps resist the efforts of an inexperienced operator. As a last proof, I might state, that I operate nearly every day with the *percuteur* without ever finding the branches give way.

3dly. During the percussion does the instrument vibrate so as to cause considerable pain to the patient?

This objection is a very natural consequence of the series of ideas which must have been formed concerning an operation, consisting in conveying into the interior of the bladder the powerful impetus of a hammer. Were I not the author of this operative process, it would be one of the first arguments that I should adduce in objection to it. I was not therefore surprised when the members of the Institute, who were examining the instrument, not only expressed doubts as to the fear of vibration, but intimated also the possibility of the entire instrument being made to move forward by the blows of the hammer. In order to reply to these objections, I put the instrument repeatedly to the test before the members themselves; and these trials were so much the more convincing and conclusive from being made comparatively with instruments acting by *progressive trituration*, such as the *perce-pierre*. To the surprise of the examiners, the result of them was, that with the latter instrument, which they supposed acted upon stone without causing much vibration, the bladder of the subject and water it contained, were subjected, during the action, to a considerable tremulous motion,* whilst hard blows dealt on the *percuteur* with a hammer, produced no vibration whatever. Although this may at first cause surprise, a moment's reflection will render it clear. An instrument acting progressively on a stone can only work efficiently by a rotatory movement, produced by the agency of a pulley; this movement must necessarily cause vibration, which is transmitted more or less to the organ in which the operation takes place. By *percussion*, on the contrary, the only manner in which the instrument can move is from before backwards, and no vibration can ensue, since no lateral movement takes place. The movement from before backwards, however, is completely obviated by placing a steel wedge, constructed for the purpose, between the anterior curve of the steel bar forming the *fixed point*, and the corresponding angle of the mortice in which this bar plays. I think I shall have given sufficient proof of the immobility of the *percuteur* during the percussion, when I have added, that during the action of the hammer, the patients never give the least sign of suffering, but, on the contrary, all agree in stating that this is precisely the moment of the operation which is most exempt from painful sensations, and that it is considered, as it were, a moment of rest. This will be fully concurred in by those who have seen the instrument act outside the bladder, and who have placed their hand in contact with that portion of it where the comminution of the stone is effected, and have felt its perfect immobility. If, therefore, the hand placed in close contact with the instrument does not experience the least disagreeable sensation during the action of the hammer, how can the bladder be in any way exposed, which, by being previously distended with water, is in no sort of contact with the instrument?

4thly. During the percussion, do the fragments of stone fly against the bladder and injure it?

The bladder being filled with water, we should be rather inclined to consider this objection as chimerical; for, physically, a body propelled through this fluid, is almost entirely deprived of the power of injury. This will be clear to those who, under the water, have tried to deal a blow with any

* It must here be remarked that, in the experiments to which I allude, M. Leroy (D'Etoiles) was the operator, and he made use of the *rectangular bed* and *fixed point*. When the *perce-pierre* is employed with a *chevalet* (hand-vice), held by an assistant, there not only must be very considerable vibration, but with such unsteady means of support as a hand, the entire instrument must unavoidably be in continual movement.

degree of force; the most violent efforts have but a trifling effect. I shall not dwell any longer on this point, having said enough to prove, that if even pieces of stone were propelled by force with the blows of the hammer, no inconveniences would result from it; but this *propulsion does not take place*, and consequently the objection of the possibility of injuring the bladder falls of itself, since there can be no effect without cause. The members of the commission paid special attention to this point, and I proved to them, by acting on hard and very dry calculi, well calculated to exemplify the above-named objection, that the broken pieces fall from the instrument almost perpendicularly, and receive scarcely any other impulse than that of their own specific gravity. I also proved, that these fragments are only projected at all forcibly when I begin the percussion by hard and repeated blows, without having first by softer strokes, loosened the layers of the stone, and so prepared it to fall more readily to pieces. In operating, I always commence by gentle blows, so that this objection is in every way answered.

5thly. Is the *percuteur courbe à marteau* ill calculated to seize the stone?

I cannot better answer this objection than by following the example of a celebrated philosopher, who, to prove the existence of motion, *walked*. Since *lithotripsy* has engaged attention, some persons have received, and still retain, an impression that an instrument with *two branches* cannot seize a stone properly, so as to break it down, although I have repeatedly proved the fact, by the use of the *brise-coque*, that such instruments are much better calculated to seize oval and flat calculi, than those composed of a greater number of branches. The percuteur, which has been similarly found fault with, furnishes another proof, that of all instruments, those with two branches *seize* calculi (especially oval and flat ones) with the greatest rapidity and facility; *hold* them with the greatest security, and require to accomplish this, the least searching and manœuvring, and are consequently least injurious to the organ. This objection, having some weight in Paris, it was named to me by the Members of the Institute, but I unanswerably proved to them that it was unfounded, by first making this instrument act before their eyes, and seizing a stone 20 or 30 times in the space of a minute, and afterwards by renewing the experiment in the bladder of a dead subject, and seizing *immediately* at their command the stone. These trials were repeated 50 or 60 times before the members of the committee, with equal success. I would state as a further proof, that almost every day some of the most distinguished surgeons in London see me operate with the *percuteur*, and I do not think any of them can say that they ever saw me introduce it into the bladder without directly laying hold of the stone. Proofs of this have lately been given *publicly* at the Greenwich Hospital, in a case which I treated there with complete success. This fifth objection is therefore fully cleared up.

6thly. Are curved instruments useless, and do straight ones always suffice?

Admitting that it is always possible to introduce straight instruments into the bladder, this is no reason whatever why curved ones should be deemed unnecessary; for, in *lithotripsy* there is more to consider than the mere introduction of an instrument; it is requisite, to seize the stone easily, to comminute it rapidly, and avoid irritating the bladder. It is to this end chiefly that the *percuteur* was constructed, and a surgeon, accustomed to use it, will I think find, that he derives these advantages from it. The curve, which is merely a *consequence* of what the instrument was intended to fulfil,* is a secondary property, although it becomes one of the greatest importance when straight instruments cannot be introduced into the bladder, or at least only with difficulty, and consequently with pain and danger to the patient. To contest the existence of such cases is confessing a want of experience, for they are by no means rare. I myself have met with eight or ten such cases, some of which I could not treat from want of a curved instrument. With this letter I have forwarded proof of this, in the case of a patient in whose bladder it was impossible to introduce a straight instrument. The curve is therefore a useful and valuable property of the *percuteur*.

7thly. Can this instrument be employed only in cases of small friable calculi?

Such an objection as this could only be made by those who judge at once, and do not take the trouble to perform trials, so as to speak from experience; or else by those who have made essays with imperfect instruments, and conclude accordingly. I sufficiently proved how completely inapplicable the objection was to the *percuteur*, by the experiments which I made before the members of the Institute; in three experiments I completely pulverised very large, and extremely hard calculi, with which I was furnished by Dr. Souberbielle, who, some years before, had extracted them by lithotomy. The cases in which I have operated, and daily operate, tend still more strongly to do away with this imaginary objection. When a stone, besides being very large, is round—a shape which pre-

* Refer to my work, *Physical Properties of Calculi*, page 155.

sents very great resistance to the action of the *percuteur*, an excavation is first made with the *evideur à forceps*, and when it is thus reduced to a sort of thick shell, it yields readily to the blows of the hammer.

9thly. Does this instrument produce fragments and no powder, and thus render the evacuation of the stone more difficult?

The Baron Dupuytren particularly named and attended to this point; and I proved to him and his colleagues, by repeated operations on the dead subject, that a much larger quantity of powder was produced by the action of the *percuteur*, than by that of instruments *acting progressively* on the stone; for when a portion of stone is seized between the beaks or branches of the *percuteur*, it is not only crushed, but the considerable portion of it which is directly between the branches, is forced out by the blows of the hammer in a sort of pulp, which, when dried, is found to be fine powder; the fragments formed are, in general, smaller, less numerous, and more regular in shape, than those produced by any other instrument; the reason of this is, that the effect of percussion on a calculus is to loosen, and at last completely to disjoin, its layers, and in this manner cause it to fall to pieces; and when these pieces are again taken and acted upon by the instrument, they usually tend, by their composition, to assume a cubical shape. With the *percc-pierre* it is very different; for it is clear that the pieces which result from the action of an instrument that bores holes in a stone, and consequently leaves sharp edges, must necessarily be both sharp and irregular in form.

Such are the observations which I think right to publish, with the hope of doing away in some measure with the false ideas which may have been formed respecting this new and important operative process. I hope I have been sufficiently clear and concise to be understood by all; and that I have been able to render more evident the value of an instrument which, by the numerous successful cases in which it has already been employed, and the testimony given in its favour by so many distinguished members of the profession who have seen it in use, seems to deserve attention, and call forth the careful examination of men of science. I have the honour to be, Sir,

Your obedient servant,

BARON HEURTELOUP, D.M.P.

1, Vere Street, Cavendish Square.

July 28, 1832.

Case of Mr. S., alluded to above.

Mr. S., residing in London, fifty-nine years of age, rather stout, but of a good constitution, whilst in the country about a year ago, observed, after a day of hunting, that his urine was deeply tinged with blood, and that he voided it with difficulty. This continued for three or four days, accompanied by a frequent and urgent desire to make water. Mr. S. consulted his medical attendant, who, by an appropriate treatment, subdued this irritable state. Four or five months afterwards Mr. S. had a second attack, but of a more serious nature. Besides considerable hemorrhage, there were fever and considerable pain on making water, and the urine became thick, offensive, and catarrhal; these symptoms continued with little abatement for nearly a month, when Mr. S. came to London and consulted Dr. Prout, who allayed the pain and irritability of the bladder, and obtained an almost complete cessation of the catarrh; Dr. Prout then called Mr. Brodie in consultation, who sounded the patient, felt a stone, and conjointly with Dr. Prout, did me the honour to place the patient under my care.

When I proceeded to sound Mr. S. with the *recto-curvilinear catheter*, I found that I could not make the instrument advance any further than the triangular ligament; I placed the catheter in Mr. Brodie's hands, who found it equally impossible, notwithstanding the most varied and careful manœuvres to introduce it into the bladder. I gave up making any further attempts with that instrument, and concluded that it had failed on account of its small diameter causing its extremity to become entangled in the folds existing at the superior part of the membranous portion of the urethra; I therefore employed a more voluminous instrument, but with precisely the same curve as the *recto-curvilinear catheter*, and it immediately passed into the bladder. I distinctly felt a moveable calculus of small size. The bladder retained a considerable injection of water, but the *bas-fond* was exceedingly narrow; so much so, that I could not turn the curve of the catheter, either to the right or to the left; the instrument had not more than an inch play from the anterior to the posterior part of the bladder, and sometimes this movement was completely prevented. When the instrument was drawn towards the neck, with the curve directed upwards, the hand experienced a sort of shock such as would be caused by the angle of the curve sliding down a sort of *elevation* or *step*. Wishing to ascertain whether a straight instrument could be introduced, I employed a smooth metal sound of about three lines in diameter; but in spite of all the most persevering and careful manœuvres, I could not succeed in passing it into the bladder; its point always struck against the lower part of the prostatic portion of the urethra,

where there existed the sort of elevation like a *step*. Finding that this patient could not admit straight instruments, I considered myself fortunate in possessing the curved *percuteur*.

With that instrument I thought I might undertake the case with confidence. The operation was performed three days after the sounding; the stone was immediately seized and broken with the hammer; two or three fragments were afterwards taken and comminuted, and the instrument was withdrawn. An injection brought away a considerable portion of detritus, and Mr. S. voided in the course of the three succeeding days the remaining portions; I introduced the *percuteur* a second time without feeling the smallest fragment; three days after I sounded him with the greatest care with the same result, and, finally, Mr. Brodie having also sounded him without feeling any stone, we had the satisfaction to inform the patient that it was completely extracted.

Mr. Brodie and Dr. Prout were present during all the steps of this case.

Reflections.—This is certainly a case worthy of remark, inasmuch as patients in whom there is so decided an obstacle to the introduction of straight instruments are seldom met with. The end of the straight-sound invariably struck against the prostatic *cul de sac*, and notwithstanding every possible manœuvre to disengage it, I could never succeed in surmounting this obstacle. This affords another proof, that the curve in the *percuteur* is not one of the least important of its advantages, since without it, in such cases, its other properties would be useless, for before we can comminute a stone, it is necessary to arrive in the organ that contains it.

The stoutness of Mr. S. was the principal cause of the difficulty in introducing the straight instrument; and, in most cases, it is the principal cause. The bladder, in these patients, is very much raised in the pelvis, by the quantity of fat which accumulates in the lower part of this cavity; the neck of the bladder is consequently also very much raised, and as the perforation through the triangular ligament does not alter its position, it evidently follows, that a straight sound, which has passed through it, is inconveniently directed to pass through the neck of the bladder. If, in these cases, the prostatic *cul de sac* be at all enlarged, either naturally or by the enlargement of the prostate, it is clear, that the difficulty of reaching the bladder with a straight instrument is the greater. It is in such cases that the curved *percuteur* becomes of absolute necessity.

THE LANCET.

London, Saturday, August 4, 1832.

A Correspondent inquires why we have been so silent respecting the second irruption of cholera in London, and speaks of the omission as a dereliction of public duty on our part. He assumes that as soon as the second epidemic commenced we were bound to announce, and comment on, its existence, with the double object of warning foreign nations against the dangers of trading with us, and of contributing towards domestic safety by affording our fellow-citizens a true view of the magnitude of the existing danger. We proceed to state the reasons of our silence, and to reply to the grounds on which our correspondent supports his complaint.

On the first occurrence of malignant cholera in London, our readers will remember with what rabid mouths and "*a la lanterne*" outcries all those were assailed whose principles or occupations involved them in announcements of the actual state of the disease. Every man who dared to speak the truth was pointed out as the enemy of his country and its commerce, and was abused as energetically as if he had been the actual introducer of the pestilence. The reporters of cases were openly accused of forgery, and of being parties to a system of bribery; nay, it was even hinted in a zealous mercantile periodical (not the *Times*) that the cholera hunters, for want of genuine game, were trying what they could do with poison. Well, the disease worked on in its steady way. It silenced, effectually, a few of the libellers, and the rest grew ashamed of their occupation, and sneaked into their native darkness out of the basking places which they had occupied in the sunny columns of the *Times*, and other public journals. Meanwhile the

epidemic departed,—but to return in a few weeks. As soon as we became acquainted with its second visit, we determined to ascertain, as far as possible, how far the suppression of reports or notices of the disease would influence the extent of public alarm. This experiment we have now fairly tried. Possessing ample means of procuring the most correct information from every parish in the metropolis, we were soon enabled to perceive an almost incredible extent of exaggeration and terror on the part of the public. One gentleman of undoubted veracity, but large imagination, about ten days since, accosted us with paleness and fright in his countenance, and informed us that he was just leaving town "because three hundred persons had died of the cholera in his parish the day previous." Gentle and simple, young and old, rich and poor, all talked of nothing, dreamed of nothing, but the cholera: many persons, we know, actually frightened themselves into the disease, and all this at a time when the deaths from cholera did not exceed a score and half a day in the whole metropolis.

If then we admit, what cannot be denied by any sane individual, that public terror is an evil of the most serious character during the prevalence of this disease, that its influence on the public health is of the most pernicious description, it follows that the wisest plan is the publication of reports stating the true account of the evil.

But independent of the salutary effect produced by such reports in checking alarm while the disease is moderate (and no pestilence is so destructive of which rumour will not magnify its virulence tenfold), they are calculated also to work much good, even under more auspicious circumstances. While suppression of the truth is practised, the heedless traveller falls into the theatre of contagion and miasm, without any warning of his danger. In an infected town, the inhabitants are not enabled to distinguish the healthy from the insalubrious quarters. Lastly, in withholding correct information, we are unable to trace the pathways and habits of the malady, and allow it to quit the country without those statistical records which are so invaluable in the scrutiny of all pestilential diseases.

For these reasons, and we could add many others, we agree with our correspondent as to the necessity of breaking silence, and as to the propriety of the publication of official reports of the state of the epidemic. His Majesty's Government has, however, determined to withhold them for the present, on what grounds we are utterly at a loss to understand. The quarantine of Spain is as strict as ever on the port of London. Holland is now infected from north to south, *despite the most rigid quarantine*. The disease has reached America, and France has all but abolished her sanitary restrictions. So that with Spain we cannot be worse, and with France, Holland, and America, we are upon equal terms. At any rate, it is now quite apparent that quarantine laws can no more restrict cholera than they can keep off a descent of congreve rockets. Where then, we may ask, is now the advantage of the secresy which is practised by the Ministry?

With respect to the document issued by the market gardeners, which we publish in another page, we are bound, on consideration, to say, that it by no means proves the case they desire to establish, inasmuch as all the *fruit-fed* individuals whom they specify, have hitherto lived beyond the bounds of the epidemic influence. Had a similar number of exemptions been recorded *in a place infected with cholera*, the evidence would then be conclusive for the gardeners. At present it is by no means sufficient to induce us to overlook the fact, that in three-fourths of the numerous cases which we have met with in the last month,

fruit was clearly ascertained to have been the *exciting cause* of the attack.

We are happy, however, to be able to add, that the disease is so much on the decline in the metropolis, that in all probability, the early autumnal fruits may be consumed with the usual impunity. *The deaths in the metropolis have not for the past week averaged more than thirty a day*, and there remain now, under treatment, not more than 350 cases. The document to which we have alluded, however, may fairly be allowed to have its effect in moderating the alarm felt in districts which the cholera has not yet visited.

THE correspondence which has lately passed between Dr. MACKINTOSH of Edinburgh and certain of the "powers that be," relative to the approaching appointment of a professor to the chair of medical jurisprudence in that University, has been laid before us. With nine-tenths of the pamphlet we have nothing to do; so much of it, at least, is altogether of a personal nature; though we have no objection to make to the form which the question therein assumes. Dr. MACKINTOSH is the champion of a free and just election, and as every attack and defence must have a leader, we are rather gratified than otherwise at the spirit and boldness, though marked by much egotism, with which the charge has been headed by him—in print, at any rate. Dr. MACKINTOSH is a very able and zealous man, and can fight energetically when his sword is drawn. We disregard, however, after a careful perusal, nine-and-twenty of his pages, and from the thirtieth select the only passage which awakens our attention to this controversy. It is contained in a petition to the House of Commons from eleven lecturers on different branches of medicine in the northern capital, and furnishes the public in a short space with the whole gist of the question in dispute.

"It is with the deepest and most unfeigned regret that your petitioners find themselves obliged to call the attention of your Honourable House to the late appointments which have taken place to those Professorships in the University of Edinburgh which are in the gift of the Crown. Your petitioners are prepared to prove, that the late appointments took place *in a hurried and clandestine manner*, thereby preventing the claims of other members of the profession from being duly considered; and it is the general belief of the profession that the chairs were bestowed *more as the reward of political than of medical zeal*. Your petitioners approach your Honourable House, not for the vain purpose of making complaint, but to pray that, in your wisdom, you will *institute an inquiry into the late appointments;* that you will take such measures as to prevent improper appointments *in future;* and that as there is at present a vacancy in one of the medical chairs, your Honourable House will take measures *to prevent the interests of medicine and science being sacrificed to political intrigue*. And your petitioners will ever pray. (Signed)

"JOHN LIZARS, Prof. of Surgery to the R. C. Surgeons, &c.
JOHN THATCHER, M.D., Lect. Midwifery.
R. KNOX, M.D., Lect. on Anatomy.
J. A. ROBERTSON, M.D., Lect. on Mat. Med.
ALEX. J. LIZARS, Lect. on Anatomy.
W. FERGUSSON, Lect. on Surgery.
JOHN MACKINTOSH, M. D., Lect. on Pathology and Prac. Phy.
JOHN MURRAY, M. D., Lect. on Chemistry.
T. J. AITKIN, M. D., Lect. on Instit. of Med. and on Mat. Med.
W. CAMPBELL, M.D. Lect. on Prac. of Phy. and Midwifery.
JOHN FLETCHER, M.D. Lect. on the Instit. of Med."

This is a most serious charge, but what is there in it that is rare? Why "the interests of medicine have been sacrificed to political intrigue" in this country for centuries. The immediate cause, however, of this protestation is a confident rumour that the chair of Medical Jurisprudence, vacant by the appointment of Dr. CHRISTISON to the chair of Materia Medica, is to be given to Dr. MACLAGAN, through the influence of the leading Whigs of Edinburgh, now in office, whose interests at the various elections which have taken place for some years back, the Doctor has had many

opportunities of warmly and efficiently supporting, and whose political views he has ardently supported at the public meetings. Warned by the past, the lecturers are now in arms, and in anticipation of an undue election, petition the House of Commons to interpose in this particular instance between the unholy connexion which politics and science have so long maintained.

Of Dr. MACLAGAN we may add our opinion, that with the desirable qualifications of being a good speaker, and a man at any rate of liberal *professions*, he wants almost all the most important requisites for filling such a chair as that to which his advancement is threatened. The appointment of Dr. MACKINTOSH would scarcely be open to any objection but that of the mode of election. Let us hope that "the Crown" will reconsider its present intentions.

THE pamphlets of Drs. CRAIGIE and LATTA, of Leith, have reached us. Dr. CRAIGIE, in a letter on the treatment of the cholera which he published on the 14th of June in the *Edinburgh Courant*, did not make that "honourable mention" of Dr. LATTA, as the first physician who tried venous injections in cholera, which Dr. LATTA and his friends considered to be due from one who was writing at that particular time, and under existing circumstances. The omission was accordingly noticed in an anonymous letter in the *Edinburgh Observer*, as very unfair towards Dr. CRAIGIE. This letter contained remarks which Dr. CRAIGIE, in a subsequent epistle, spoke of as "false and unfounded." The writer of the anonymous letter proved to be Mr. JOHN MITCHELL, Jun., a ship-broker, of Leith, who confessed the authorship for the purpose of "demanding an explanation" from Dr. CRAIGIE as to the above two offensive words. An interview took place, casually, in the street, when a trifling assault with a child's whip was believed by Dr. CRAIGIE to have been committed on him by Mr. MITCHELL, who is an intimate friend of Dr. LATTA. The parties were afterwards bound over to keep the peace, although (most unaccountably to the reader) Mr. MITCHELL denies ("does not admit") that any assault took place, declaring "that he had not any whip in his hand when he asked for the apology."

This avowal certainly puts to rest the whole affair, but it is one which a peruser of the pamphlets is little prepared to meet with only in the last but one of forty-two pages.

We are disposed to doubt the justness of the original quarrel; at any rate its right to be made a subject for public discussion. Our impression is, that although Dr. LATTA may have thought he had reason to be surprised at the omission complained of, Dr. CRAIGIE did not commit an intentional offence. As for the part taken in the affair by Mr. MITCHELL, the right of a strictly anonymous correspondent to assume that he is personally affronted by any man, is perfectly new to us, and one which we think no court of honour would for a moment allow. We are glad the assault is disavowed. Had it taken place, it would have been an unwarrantable outrage both on the man and the physician. Dr. CRAIGIE was not bound to carry his inquiries as to the name of the author farther than he did.

In perusing the letters which have passed, we recollect so little that is offensive uttered by the two physicians, that either party we are sure may advance the hand of friendship without the slightest compromise of personal dignity. As for the neglected compliment to Dr. LATTA, that gentleman can afford to lose it, and no man will think so more readily than his opponent.

The importance of meteorology as a constituent in the etiology of diseases has rarely been duly appreciated by the medical profession; the most ordinary occurrences of life, however, afford us abundant proof of the great influence exercised by atmospheric changes on health. We advert to the subject here, to recommend to the reader's notice the essay of Mr. James Espy on meteorology in our present Number. It is highly ingenious, and points out accurate and easily attainable means for investigations which are replete with interest to medical men, and require but little expense or trouble to conduct them. We hope to attract attention to this much-neglected branch of science.

The length of the lecture on the nervous system of by far the most important of all our domestic animals, which we this day publish, precludes us from giving in our present Number several valuable papers which we had marked late in the week for insertion. The extent of Mr. Youatt's luminous and admirable sketch of a most intricate, and, usually-esteemed, mysterious subject, will be regretted, however, by none of the thousands of attentive readers who are following this highly philosophic lecturer in his course, through the medium of our pages. It would have been great injustice to all parties, indeed, to divide the lecture at any part. To allow its completion, in fact, in the most perfect manner, we delayed its publication from our last Number, and now submit it to public perusal as an excellent example of deep research, original observation, acute reflection, and a terse and clear style of expression, happily combined within the compass of one hour and a half's discourse, on a highly abstruse and hitherto most wretchedly investigated subject. The lecture, in truth, is quite a model in its way, and, from the care and laborious investigation which distinguish it, is one of the most valuable which has ever been presented to the readers of this journal. Indeed the entire of this *unique* course, demands the high estimation of the profession, and its value, we are rejoiced to find is, as regards the portion already published, fully appreciated by every medical reader who derives advantage in his daily labours from the services of the invaluable domestic animal which is the chief subject of the learned lecturer's precepts.

AN INQUIRY INTO THE

ORIGINAL COUNTRY

OF THE

SOLANUM TUBEROSUM (Potato).

By John Edward O'Reilly,
A.B.M.B. & M.R.C.S.L.

The writer of the article Potato in *Rees's Cyclopedia* asserts, that it was brought into Europe from Virginia by Sir Walter Raleigh in the year 1623. This is probably on the authority of Wildenhow, who makes the like statement; and Sir Joseph Banks, in a paper published in the Transactions of the Horticultural Society, arrives at almost the same conclusion, save that he fixes the year 1586. Humboldt, in his New Spain, book the 4th, also appears to think that the first English settlers found it in Virginia; and to these authorities may be added the illustrious name of Jefferson. (*Notes on Virginia.*)

The views which I shall attempt to sustain in this paper are, first, that the potato was wholly unknown to the original inhabitants of Virginia; secondly, that Peru, and probably Chili, were the countries where the first European visitors of America found it; and, thirdly, that there exists some probability, very slight I admit, that this plant was not quite unknown to the ancients.

The principal arguments in favour of its having been found native in Virginia are—That Thomas Herriot, who visited Virginia with the first discoverers, describes the potato as the "openawk;" that Gerard, in his Herbal, published in London in 1586, describes the potato, or, as he terms it,

openawk, as being an inhabitant of Virginia; and, what I do not find mentioned by Sir Joseph Banks, John Parkinson, in a very elaborate work entitled Theatrum Botanicum, published in London in 1640, corroborates Gerard.

The statements of Wildenhow, and of Rees' Cyclopedia, are manifestly inaccurate. Raleigh having been beheaded in Old Palace Yard on the 29th of October, 1618, could not bring potatoes into Ireland in 1623; and indeed Dr. Barton Smith, of Philadelphia, asserts that Raleigh never was himself in Virginia.

I have very carefully examined Herriot's work, published in De Bry's collection (Ed. Frankfort on the Main 1690), and do not think the openawk described by him is the potato; and I am the more confirmed in this, as, though Herriot gives no figure of this plant, yet De Laet does, in his Novus Orbis (*Editio apud Elzevirios* 1633), which assuredly is quite a different plant from our potato; moreover it is not at all proved that Novembega, where Herriot found the openawk, was Virginia.

Gerard and Parkinson were both compilers, and participated in the mistake, as they both call the potato the openawk.

Mr. Jefferson does not state his authority. He says, "Most probably they were natives of more southern climates, and handed along the Continent from one nation of savages to another." Now this account is at variance with Humboldt's positive assertion (lib. cit.), "That there is not a single fact connecting the history or races of the two Americas."

Colonel Benjamin Hawkins, who resided as United States agent to the Creek Indians, assured Dr. B. Smith (Nicholson's Journal, vol. xxviii) that the Indians say they received it from the Europeans; and Capt. John Smith, who visited Virginia in the early part of the 17th century, says that the potato was brought to that colony in 1613 by the ship Elizabeth from England.

I shall now proceed to demonstrate where the first settlers from Europe in America found the potato. Peter de Cieza, who resided seventeen years in Peru, and who wrote in 1541 (translated from the Spanish by Stevens), says, in describing the neighbourhood of Quito, "Besides the maize or Indian wheat, there are two other sorts of food natural to the country; the one they call pappas, which is like large pig-nuts, and when boiled are as tender as large chestnuts, and has neither shell nor core, but grows under ground like the pig-nuts aforesaid. This root bears a flower exactly like a poppy."

This account of Cieza is confirmed, as to its principal bearings, by the Inca Garcilasso de La Vega, whose mother was a native Peruvian. (Royal Commentaries of Peru.) The authorities on this subject are very numerous, and can be all found in the Fagel Library of Trinity College, Dublin, and are all corroborative of the account of Cieza. I shall merely mention some of the principal.

Gulielmi Pisonis Indiæ utriusque de re Naturali et Medica, libri xxx. Historia Nova Plantarum, Animalium et Mineralium Mexicanorum, a Francisco Hernandez Histoire Generale de F. Lopez de Gomara.

Mr. Humboldt found in Chili a wild potato differing certainly from ours, but cultivation frequently makes a very great change in plants.

The third proposition is founded on the description given by Theophrastus* of a plant which he calls the arachidna (*αραχιδνη*). His description wonderfully applies to the potato, but that he says it is leafless. (*φυλλον δε ουτεροφ[illegible]χει τουτων ουδ' ομοια τοις φυλλοις.*) This, Clusius, an eminent botanist of the 16th century, endeavours to get over, by supposing an inaccuracy in Theophrastus, and stoutly maintains the arachidna and papas punanum (potato) to be one and the same. (*Rariorum Plantarum Historia.*)

The arachidna is also described by Pliny, but he copies Theophrastus literally. (*Plin. Secund.*, lib. xxi. cap. 52, *cura Valpy.*) His commentator styles it—"Herba hoc ævo ignota."

Since writing the above, I find that Don Joseph Pavon, in his Flora Peruviana, fully supports Cieza, and the other authorities cited above, as to Peru and Chili being the original country of that plant, which many a one of my poorer countrymen may address—"Salve, magna parens."

Dublin, June, 1832.

* Three hundred years before the Christian era.

CORRESPONDENTS, &c.

Mr. Joseph Hickman. The works of Beck and Christison. We do not know of a good work on the subject of the analysis.

ERRATA.—In Mr. Jackson's case of melæna page 498, line 2, for "quorum" *read* quarum; line 35, for "animat" *read* sumitur; line 45, for "aque linnam" *read* aquæ cinnam; col. 2, line 9, for "enemata" *read* enemate. These errors occur only in a part of the impression.

In the note at the end of Dr. Elliotson's lecture, page 518, line 13, the comma after the word "absence," should be transferred to the close of the word "intervals," in the next line. The error is in a few copies only.